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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS, ABSTRACTS OF PAPERS, AND

REPORTS OF THE PROCEEDINGS

OF THE SOCIETY

FROM NOVEMBER 1883 TO NOVEMBER 1884.

VOL. XLIV.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLIV. NOVEMBER 9, 1883. No. 1.

E. J. STONE, M.A., F.R.S., President, in the Chair.

Prof. S. P. Langley, Allegheny Observatory, Pennsylvania;
Prof. J. A. C. Oudemans, the Observatory, Utrecht;
Prof. P. Tacchini, Collegio Romano, Rome; and
Prof. E. Weiss, the Observatory, Vienna;
were balloted for and duly elected Associates of the Society.

James S. Cooke, Springfield, Gomersal, near Leeds;
Alexander von Gothard, Herény Observatory, Hungary;
Eugen von Gothard, Herény Observatory, Hungary;
Capt. Richard Ladd, F.M.S., 106, Sandgate Road, Folkestone; and
Robert Rawson, Assoc. I.N.A., Havant, Hants;

were balloted for and duly elected Fellows of the Society.

Note on the Greenwich Mean Tabular Errors of the R. A. of the Sun. By E. Dunkin, F.R.S.

In the table of mean tabular errors of the Sun in longitude given by Mr. Stone in his paper on "An Explanation of the Principal Cause of the larger Errors at present existing between the Positions of the Moon deduced from Hansen's Tables and from Observation," &c. (*Monthly Notices*, vol. xliii. p. 337), the changes appear so remarkable, especially in the almost sudden increase in the errors after the year 1872, that it is highly important that any explanation of this apparent increase in the tabular errors of longitude, or of the R.A. of the Sun, should be at once placed on

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record. Mr. Stone considers that the general increase is clearly progressive, amounting roughly to 0″·052 per annum, and he believes that it is the effect of the change of unit adopted by Le Verrier in his Solar Tables.

Without wishing to enter into a controversy on the subject of Mr. Stone's paper, I think I may venture to offer another explanation, which will also clearly show that this apparent positive increase in the observed tabular errors of the R.A. of the Sun may be accounted for as being the effect of "personality" in the observers employed in making the observations. It so happened that between the years 1872 and 1875 several important changes took place in the regular observing staff at Greenwich. Observers who were in the habit of systematically giving large negative errors were succeeded by others who, on the contrary, give large positive errors. The existence of a peculiar personality in observing the limbs of the Sun and Moon has been known to me for many years, long before the preparation of my two papers on personality in observing transits of the first and second limbs. In these papers I have conclusively shown that even experienced observers differ widely in their methods of observing the limbs of the Sun and Moon (*Monthly Notices*, vols. xxix. and xxxv.)

The numbers given in the following table for each of the principal observers show how much the mean tabular errors of the R.A. of the Sun have been affected by the introduction of new observers. These numbers require no explanation further than to state that all the tabular errors in R.A. of the Sun have been extracted, without exception, from the "Astronomical Results" contained in the *Greenwich Observations* for the years from 1864 to 1881. These errors have been classified for each year into observers' groups. All the errors deduced from the observations of the computers and other occasional observers are included in one miscellaneous group, but any evidence obtained from this group is of lesser importance, as the observers are never in any two years the same, and many of them have not attained the trained experience of the assistants. In the following table the yearly mean errors derived from the observations of all the occasional observers are inserted, but no attempt has been made to ascertain the amount of their separate personalities, though the errors themselves show that personality exists in some instances in a tolerably large degree.

In the following tables the initials of the observers D, E, L, C, JC, AD, T and TL, are respectively those of Mr. Dunkin, Mr. Ellis, Mr. Lynn, Mr. Criswick, Mr. Carpenter, Mr. Downing, Mr. Thackeray, and Mr. Lewis. The mean tabular errors are expressed in seconds of arc.

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Mean Tabular Errors of R.A. of the Sun.

Year.	D Tab. Error of R.A.	No. of Obs.	E Tab. Error of R.A.	No. of Obs.	C Tab. Error of R.A.	No. of Obs.	JC Tab. Error of R.A.	No. of Obs.	Miscellaneous. Tab. Error of R.A.	No. of Obs.
	"		"		"		"		"	
1864	−0·07	17	−0·25	23	+0·19	28	−0·97	24	+1·03	30
1865	−0·09	21	+0·18	24	+0·55	28	−0·03	19	+0·38	29
1866	−0·34	19	−0·87	20	−0·10	16	−0·67	21	+0·67	33
1867	−0·36	14	−1·48	16	+0·84	17	−0·79	18	+0·26	27
1868	+0·13	21	−0·88	21	+0·21	25	−0·19	19	+0·84	45
1869	+0·55	14	−0·58	18	+0·51	18	−0·17	18	+0·82	30
	L									
1870	+0·21	18	−0·46	20	+0·45	23	−0·56	28	+0·75	37
1871	−0·03	28	−0·17	15	+0·18	16	−1·17	15	+1·42	39
1872	+0·79	25	−0·91	23	−0·07	19	−1·29	16	+0·39	37
							AD			
1873	+0·91	21	−0·60	20	+0·33	20	+0·61	29	+0·90	34
1874	+1·09	22	−0·34	12	+0·52	20	+1·72	20	+0·62	32
			T							
1875	+1·08	17	+0·15	15	+0·45	15	+0·84	15	+0·97	49
1876	+1·33	12	+0·20	22	+0·36	16	+0·64	24	+1·08	38
1877	+0·24	9	+0·08	13	+0·79	13	+0·42	16	+1·06	34
1878			+0·78	12	+0·33	14	+0·21	14	+1·05	45
1879			+0·66	13	+0·08	15	+0·31	15	+1·29	34
1880			+0·69	20	+0·19	17	−0·09	27	+1·53	51
	TL									
1881	+1·14	27	+0·52	19	+0·22	17	+0·48	24	+0·96	33

From 1878 to the end of 1880, owing to the illness and subsequent resignation of Mr. Lynn, three assistants only made regular observations with the Transit Circle, which accounts for the rather larger number of observations made by the computers in those years.

Referring all the numbers in the preceding table to Mr. Criswick as the standard, and dividing the group of each observer into smaller groups determined either by the change of observers or when there is shown a decided difference in the value of the errors, we obtain a series of comparisons between Mr. Criswick and each observer, and indirectly between any two observers; and thus corrections can be deduced to be applied to the mean of all the errors for the years between which a change of observers took place.

Period.	Observers.	Difference.	
1864–1869	D−C =	−0″·40	
1864–1869	E−C =	−1·01	Mean = −0″·90
1870–1874	E−C =	−0·78	
1864–1869	JC−C =	−0·84	Mean = −1″·02
1870–1872	JC−C =	−1·19	

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Period.	Observers.	Difference.
1870–1871	L–C =	−0·23
1872–1877	L–C =	+0·51
1873–1874	AD–C =	+0·74
1875–1881	AD–C =	−0·01
1875–1881	T–C =	+0·09
1881	TL–C =	+0·92

Assuming on an average that the number of observations of each observer is approximately about one-sixth part of the whole number for each year, corrections have been determined from these differences, and applied to form the corrected values in the third and sixth columns in the next table. It will be observed that though the discordances in the third column of mean errors are greatly diminished between the periods before and after 1873, the errors are not entirely cleared of the personality, as it still remains in full force in the miscellaneous group, which has in some years a very considerable weight in determining the yearly combined mean of the tabular errors. The following table contains the mean of the errors determined by all the observers for each year, with the resulting values as corrected for the personality due to the changes in the assistants, and also the means of the tabular errors determined from observations made by the assistants only, with the corresponding resulting values corrected for personality.

Year.	Uncorrected Mean Tab. Error of R.A. of Sun from all the Observations.	Mean Tab. Error of R.A. of Sun corrected for Personality due to change of Assistants.	No. of Obs.	Uncorrected Mean Tab. Error of Sun from Observations by the Assistants only.	Mean Tab. Error of Sun from Observations by the Assistants only, corrected for Personality due to change of Assistants.	No. of Obs.
	″	″		″	″	
1864	+0·05	+0·05	122	−0·27	−0·27	92
1865	+0·23	+0·23	121	+0·20	+0·20	92
1866	−0·17	−0·17	109	−0·53	−0·53	76
1867	−0·20	−0·20	92	−0·44	−0·44	65
1868	+0·24	+0·24	131	−0·17	−0·17	86
1869	+0·15	+0·15	98	+0·07	+0·07	68
1870	+0·13	+0·09	126	−0·12	−0·16	89
1871	+0·32	+0·28	112	−0·22	−0·26	74
1872	−0·12	−0·30	120	−0·27	−0·54	83
1873	+0·51	+0·04	124	+0·36	−0·35	90
1874	+0·80	+0·33	106	+0·57	−0·14	74
1875	+0·79	+0·30	111	+0·65	−0·11	62
1876	+0·75	+0·26	112	+0·56	−0·20	74
1877	+0·66	+0·17	85	+0·40	−0·36	51
1878	+0·75	+0·42	85	+0·42	−0·24	40
1879	+0·75	+0·42	77	+0·34	−0·32	43
1880	+0·81	+0·48	115	+0·23	−0·43	64
1881	+0·73	+0·18	120	+0·64	−0·18	87

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The effect of the change of assistants on the tabular error of the R.A. of the Sun as determined from observations made with the Transit Circle is well marked in the preceding table, especially in the fifth and sixth columns, in which the errors determined from the observations of the computers have been omitted. It may be remarked that in 1870 Mr. Dunkin was succeeded by Mr. Lynn; in 1873 Mr. Carpenter by Mr. Downing; in 1875 Mr. Ellis by Mr. Thackeray; and in 1881 Mr. Lynn by Mr. Lewis.

Some of the outstanding discordances in the third column can be explained by an examination of the separate errors as given by each observer. In 1866 and 1867 two computers had undoubtedly a considerable negative personality, while as a rule it has been noticed that the young observers have a tendency to observe either the first or second limb too early, and consequently the tabular errors in R.A. for the centre of the Sun are too great. In these years the negative tabular errors determined from the observation of some of the assistants are also larger than in other years. From 1878 to 1880, owing to Mr. Lynn ceasing to observe, it became necessary to employ computers more than usual to supply his place; and the effect of their exceptionally large errors, due to personality principally, is shown in the corrected mean errors for those three years, in which a large personality for the miscellaneous group remains. In 1872 there was a great preponderance of negative errors, the cause of which cannot at present be explained.

The only assistant who has observed continuously from 1864 to 1881 is Mr. Criswick, and as it is found from the comparison of his observations with those of the other observers that no sensible change has taken place in his method of observing the Sun during the whole period, the errors deduced from his observations ought to give some reliable evidence of a progressive increase in the tabular error of R.A., if such a change be really in progress. Dividing his observations into three groups, and also the corresponding corrected errors determined by the assistants, we have:

Period.	Mean Tab. Error of R.A.	No. of Obs.	Mean corrected Tab. Error of R.A. by Assistants.	No. of Obs.
	″		″	
1864–1869	+0·36	132	−0·19	479
1870–1875	+0·32	113	−0·26	472
1876–1881	+0·31	92	−0·29	359

These numbers are closer than might have been expected, and they indicate a diminishing rather than an increasing error.

I have already remarked that similar personalities in observing the limbs of the Moon have been shown to exist, and on referring to a comparison of the observations of different observers given in my paper (*Monthly Notices*, vol. xxxv. p. 94), it may be observed that the relative differences are about the same amount as those exhibited above for the Sun.

Royal Observatory, Greenwich,
1883, *November* 8.

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Note on the Mass of Saturn. By Prof. Asaph Hall.

(*Communicated by Rear-Admiral R. W. Shufeldt, Supt. of Naval Observatory.*)

At the beginning of my observations with the 26-inch Equatorial at Washington in 1875, the apparent difference of declination of *Iapetus*, the outer satellite of *Saturn*, and the planet was very small. It occurred to me that it would be well to observe the difference of Right Ascension and Declination of the planet and satellite, since the mean distance of the satellite would be determined chiefly from the difference of Right Ascension, and so would not depend essentially on the assumed value of a revolution of the micrometer screw. *Iapetus* was observed therefore in this manner in 1875, 1876, and 1877. The differences of Right Ascension were observed as carefully as possible on a chronograph. In 1875 each difference of Right Ascension depends on about twenty-five transits, and in the other years on about twenty transits. The average probable error of a single difference of Right Ascension is $\pm 0^s.044$. Both limbs of *Saturn* were observed; and in every year the satellite was observed in both positions, preceding and following the planet. Each difference of Declination depends on five bisections of the planet and satellite.

The observed differences of Right Ascension and Declination correspond to what Bessel denotes as x and y in his work on the orbit of *Titan* (*Astronomische Nachrichten*, Band 9); and the equations of condition are formed very conveniently by means of Bessel's formulæ. The equations for each year have been treated separately, since the periodical perturbations of this satellite produced by the Sun are quite large. The solution of the equations of condition by the method of least squares gives the following values of the mean distance of *Iapetus* from *Saturn* at the distance 9·53885.

1875	$a = 515''.594 \pm 0''.056$	57 observations.
1876	$a = 515.454 \pm 0.058$	40 ,,
1877	$a = 515.517 \pm 0.053$	31 ,,
Mean	$a = 515.522$	128 ,,

The following table shows the number of equations of condition for each year; the sum of the squares of the independent terms before solution, the sum of the squares of the residuals after solution, first by elimination, and again by substitution; the maximum residual left in the equations after substitution, and the probable error of a single equation.

Year.	No of Equat.	$[nn]$	$[nn.6]$	$\Sigma.v^2$	Max. Residual	r_1
		$''$	$''$	$''$	$''$	$''$
1875	109	172·274	27·136	27·095	1·49	±0·346
1876	78	107·453	15·806	15·803	1·31	±0·316
1877	61	110·935	8·336	8·324	1·26	±0·263

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The values of the mean distance for the different years agree so nearly that it is doubtful, I think, if the same observer can get a better determination of this quantity by this method. Should this mean value prove to be erroneous, then the error must exist in the method employed or in the observer, since the accidental errors seem to be nearly eliminated. The known variability in the brightness of this satellite when on different sides of the planet does not appear to have had any sensible influence on the result.

I have also a series of angles of position and distances observed with the filar micrometer, but the equations for these have not yet been formed.

In order to determine the periodic time of *Iapetus*, I have examined the old observations of this satellite, most of which, however, are of a very unsatisfactory character. A long series of observations made at Marseilles in 1787 by Bernard promised a good determination of the orbit at that date, and such a determination would be valuable, since in this way we could find from the observations the secular variation of the node and the periodic time of the satellite; but after a good deal of labour and many trials I have not been able to understand these observations. Lalande's interpretation of them is probably wrong, since he gets the inclination of the orbit largely in error (*Mém. de l'Académie*, Paris 1786). For the periodic time, therefore, I have used an observation by Sir William Herschel, Sept. 20, 1789, and the observations made by Sir John Herschel at the Cape of Good Hope in 1837. These have been compared with my own observed times of the conjunctions in 1880 and 1881. The adopted value of the sidereal revolution is

$$\text{Periodic Time} = 79{\cdot}3310152 \text{ days.}$$

Combining the above mean distance with this period we have data for determining the mass of *Saturn*. For *Saturn* I find

$$\rho - \tfrac{1}{2}\phi = 0{\cdot}01818,$$

and in the case of *Iapetus*, which is so distant from its primary, I have assumed that the factor

$$1 + \frac{\beta^2}{a^2}(\rho - \tfrac{1}{2}\phi)$$

is unity, this factor being the constant part of the expression, *Méc. Cél.* tome ii. p. 103. The masses of the satellites are unknown, and I have neglected the action of the Ring, since Bessel's value of its mass does not appear to be beyond question. We have therefore for the mass of the planet in units of the Sun's mass—

$$\text{Mass of Saturn} = \frac{1}{3482{\cdot}2}.$$

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There is a series of observations of this satellite by Bessel in the volumes of the *Königsberg Observations*, 1832 to 1837. *Iapetus* was compared with *Titan* by means of the Heliometer; and it would be interesting to have a determination of the orbit of *Iapetus*, and of the mass of the planet, from these measures after the method proposed by O. Struve.

Naval Observatory, Washington:
1883, *Sept.* 25.

The Disappearance of the Satellites of Jupiter.
By Wentworth Erck.

The simultaneous disappearance, predicted by the *Nautical Almanac* for the morning of the 15th, did not take place, owing to an error of twenty-four minutes in the *Nautical Almanac* time of egress of the fourth satellite, which passed off the disk just as the third satellite entered on the disk.

The following is an abstract of the notes made on the occasion with a $7\frac{1}{2}$-in. Equatorial, and powers of 100 and 250.

G.M.T. 1883, *Oct.* 14.

12	42	±	I. disappeared, being eclipsed.
13	5	0	Shadows of II. and III. conspicuous; but cannot see Sat. IV., which is on the disk.
	51	15	II. about to enter on the disk.
14	14	12	III. shadow off the disk.
	21	10	II. on the very point of entering; its disk is well defined, as is that of III., but the disk of II. is apparently not more than one-third of the diameter of that of III.
	28	10	II. has now completely entered. Though II. and IV. are on the disk, and the definition good, yet I cannot see the satellites themselves with powers of 100 and 250.
15	36	0	II. shadow off disk.
	50	55	III. first contact with limb.
	50	55	IV. off the disk, at the same moment that III. makes contact at the opposite side of the disk; III. enters on the great Southern Belt, whereas IV. emerges at a point very far S. of the Belt at latitude, perhaps, 30°.
	56	0	III. half on; IV. well of.
	59	25	III. completely on, took about 8^m to enter.
16	13	50	I. re-appearing from occultation.
	15	50	I. completely off. III. not seen on disk.
	20	0	Observations discontinued.

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Comparison of Observed Times with Greenwich Times.

		Observed.	Greenwich.	Diff.
1	III. Shadow E.	14 14 12	14 15 0	—
2	II. Tr. Ingress	28 10	33 0	5
3	II. Shadow E.	15 36 0	14 50 0	46
4	IV. Transit E.	51 0	16 15 0	24
5	III. Transit I.	59 25	15 56 0	3
6	I. R. Oc.	16 15 50	16 15 0	—

In the case of No. 3—*i.e.* the egress of the shadow of II.—I cannot at all account for the difference of 46 minutes.

Shankill, Co. Dublin:
1883, *Oct.* 24.

Jupiter without visible Satellites. By the Rev. S. J. Johnson.

The disappearance of *Jupiter's* satellites on the morning of October 15 afforded an interesting instance of the tabular errors of the fourth satellite. Instead of lasting nineteen minutes, as according to the *Nautical Almanac*, it only lasted half a minute, and possibly, with a large instrument, the satellites may never have entirely gone out of sight.

On turning the telescope on *Jupiter* at $3^h\ 45^m$ A.M. I never noticed the belts sharper defined. One satellite, the third, alone remained visible just at the edge of the planet. At $3^h\ 55^m\ 50^s$ it appeared to have vanished. Thirty seconds afterwards the fourth began to come into view at the opposite side of the planet; so that as viewed with a telescope of the size I was employing ($3\frac{1}{4}$-in. Equatorial, power 180), the planet for half a minute appeared denuded of his satellites; at $4^h\ 13^m\ 40^s$ A.M. the first reappeared.

Melplash Vicarage,
Bridport: Oct. 27.

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Elliptic Elements of Comet 1882, *I.* By F. J. Parsons.

(*Communicated by Prof. Safford.*)

The accompanying set of elements was derived from the following six normal places :—

G.M.T.	Mean R.A. 1882·0.	Prob. Error.	No. of Obs.	Mean δ 1882·0.	Prob. Error.	No. of Obs.	Time of Normal Places.
Mar. 26·5	271 36 46·9	±0·71	63	37 15 15·6	±0·41	61	Mar. 19–Apr.
Apr. 12·5	281 22 16·7	±0·20	70	49 49 15·1	±0·66	70	Apr. 4–Apr. 21
May 25·5	55 55 34·2	±0·45	40	59 36 4·1	±0·28	40	May 21–May 28
June 2·5	66 45 42·3	±0·56	18	44 36 27·1	±0·53	17	June 1–June 5
July 9·5	156 59 16·35	±0·59	27	9 45 17·5	±1·3	27	July 8–July 17
Aug. 7·5	181 46 55·5	±1·17	12	4 14 3·7	±1·3	12	Aug. 1–Aug. 16

From the 2nd, 3rd, and 5th of these normal places a preliminary set of elements was computed according to the methods given in Gauss's *Theoria Motus* as follows :

$$T = \text{June } 10{\cdot}52908$$
$$\log. q = 8{\cdot}7836381$$
$$\log. e = 9{\cdot}9999998$$

$$\left.\begin{aligned} \Omega &= 204^\circ\ 56'\ 18''{\cdot}61 \\ \omega &= 208\ 59\ 38\ 17 \\ i &= 73\ 48\ 34{\cdot}88 \end{aligned}\right\} \text{Mean Eq. } 1882{\cdot}0$$

Rectangular Equatorial Coordinates.

$$x = r[9{\cdot}9611128] \sin (126^\circ\ 22'\ 53''{\cdot}24 + v)$$
$$y = r[9{\cdot}8608368] \sin (\ 61\ 11\ 47{\cdot}56 + v)$$
$$z = r[9{\cdot}9021136] \sin (196\ 51\ 24{\cdot}51 + v)$$

With these elements, considering the excentricity equal to unity, comparison was made with all six normal places in order to make the final corrections to the preliminary orbit by means of a least-square solution.

A computation in duplicate by two independent methods gave the following twelve equations of condition given in logarithmic form.

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(x)	(y)	(z)	(u)	(v)	(w)	(n)	
9·16489*n*	9·27446	9·95933	9·08991*n*	9·46518	9·44609*n*	0·27560	= 0
8·41902*n*	8·98582	9·70896	9·97296	9·83479*n*	9·81569	0·72835	= 0
9·36391*n*	9·34675	9·97901	9·52692*n*	9·31175	9·32212*n*	8·58790	= 0
8·75669*n*	8·80395	9·44677	8 46007	9·87899*n*	9·88936	9·73240	= 0
8·47717	9·19942*n*	9·68374*n*	9·84899*n*	9·39972*n*	9·61464	0·16810*n*	= 0
9·84246	9·05283*n*	9·23382*n*	9·84595	7·49914*n*	7·71406	0·12710*n*	= 0
9·07512	9·14371*n*	9·48881*n*	9·48238*n*	8·97764*n*	9·37563	0·48150*n*	= 0
9·91297	8·85165*n*	8·76015*n*	9·49119	8·15680*n*	8·55479	0·47860*n*	= 0
9·49764*n*	9·12382*n*	9·58522	9·60400	9·44058	8·86482	9·03510*n*	= 0
8·38859	9·19260*n*	9·78999	0·11026*n*	9·44974*n*	8·87398*n*	9·23040*n*	= 0
8·99356*n*	8·99621*n*	9·62963	8·81397	9·61626	8·71295	0·65390*n*	= 0
7·44648*n*	9·07909*n*	9·77796	0·16787*n*	9·52880*n*	8·62549*n*	0·78890	= 0

Where

$$x = 10000\, dT$$

$$M \sin y = d \log. q \text{ (Briggian) or } y = \frac{dq}{q \sin 1''}$$

$$z = d\chi' = d\omega' + \cos i' d\Omega'$$

$$\sin u = ds \text{ or } de = \frac{u}{\sin 1''}$$

$$v = d\Omega'$$

$$w = di'$$

It was not thought worth while to apply weights systematically according to the number of observations, but as the last normal place seemed to have considerably less precision than the others, the last two equations were given a weight of 0·5012 log. 9·70000 as a convenient approximation to a weight of one-half. The above equations of condition give the following normal equations:—

1·3521χ	−0·2004y	−0·7279z	+0·5948u	−0·1752v	+0·0742w	−4·0124n	= 0
−0·2004	+0·2147	+0·3884	+0·2805	+0·0471	−0·0861	+1·8309	= 0
−0·7279	+0·3884	+3·2376	−0·7158	−0·0294	−0·1974	+7·4300	= 0
+0·5948	+0·2805	−0·7158	+5·1019	+0·1672	+0·5524	+0·3768	= 0
−0·1752	+0·0471	−0·0294	+0·1672	+1·5376	−1·2257	−4·7598	= 0
+0·0742	−0·0861	−0·1974	+0·5524	−1·2257	+1·3913	+1·7076	= 0

Giving the following values for the unknown quantities:—

$$x = \quad 4''.4771 \pm 1''.468$$
$$y = \quad 2·4213 \pm 4·214$$
$$z = -\ 1·4295 \pm 0·875$$
$$u = -\ 2·2662 \pm 0·877$$
$$v = +\ 10·952 \quad \pm 2·495$$
$$w = +\ 9·035 \quad \pm 2·739$$

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Substituting these values in the equations of condition gives for the sum of the squares of the residuals $[vv]=22\cdot02$. In the solution of the normal equations $[nn]=22\cdot09$.

From these unknown quantities the following corrections to the preliminary elements are found.

$$dT = +\ 0\cdot000448 \pm 0\cdot000147$$
$$d\log. q = +\ 0\cdot0000051 \pm 0\cdot0000089$$
$$d\omega' = -\ 8''\cdot024 \pm 2''\cdot644$$
$$de = -\ 0\cdot00001098 \pm 0\cdot00000425$$
$$d\Omega' = +\ 10''\cdot952 \pm 2''\cdot495$$
$$di' = +\ 9\cdot035 \pm 2\cdot739$$

These corrections give, as the most probable values of the elliptic elements—

$$T = \text{June } 10\cdot52953.\ \text{G.M.T.} \pm 0\cdot00015$$
$$\log. q = 8\cdot7836432 \pm 0\cdot0000089$$
$$e = 0\cdot99998902 \pm 0\cdot00000425$$

$\omega = 208^\circ\ 59'\ 33''\cdot72$	Mean Equinox and Ecliptic 1882·0.		$\omega' = 196^\circ\ 51'\ 16''\cdot49 \pm 2''\cdot644$	Referred to Equator.
$\Omega = 204\ 56\ 29\cdot49$			$\Omega' = 210\ 29\ 12\cdot25 \pm 2\cdot495$	
$i = 73\ 48\ 41\cdot82$			$i' = 52\ 57\ 41\cdot04 \pm 2\cdot739$	

Rectangular Equatorial Coordinates.

$$x = r[9\cdot9611023] \sin (126^\circ\ 22'\ 49''\cdot33 + v)$$
$$y = r[9\cdot8608362] \sin (\ 61\ 11\ 58\cdot04 + v)$$
$$z = r[9\cdot9021280] \sin (196\ 51\ 16\cdot49 + v)$$

These elements correspond to a period of about 400,000 years.

Recomputing the position of the comet for the dates of the six normal places, and comparing, gives the following results—the column headed "v" being the residuals obtained by substituting in the equations of condition the values found above for the unknown quantities.

	$\delta a \cos\delta$ $(c-o)$	v	$d\delta$ $(c-o)$	v
Mar. 26·5	+1·36	+1·34	+1·01	+1·00
April 12·5	−0·86	−0·71	−1·31	−1·31
May 25·5	+1·53	+1·52	+0·19	+0·17
June 2·5	−0·61	−0·60	+0·04	+0·03
July 9·5	+0·37	+0·38	−2·33	−2·16
Aug. 7·5	−0·91	−0·95	+4·30	+4·24

The planetary perturbations, being small, have not as yet been taken into account, as I intend to make a more complete discussion of the comet when all the observations have been published.

Field Memorial Observatory,
Williamstown, Mass.

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Observations of Comet b, 1883 (*Pons-Brooks*), *made at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The observations on September 5 and 26 with the South-East Equatorial (aperture 12·8 inches), and those on October 21 and 29 with the East or Sheepshanks Equatorial (aperture 6·7 inches), were all made by taking transits over two cross-wires at right angles to each other, and each inclined 45° to the parallel of declination.

Observations of Comet b, 1883 (*Pons–Brooks*), *with the S.E. Equatorial. Aperture 12·8 inches.*

Greenwich Mean Solar Time 1883, Sept. d h m	Obs.	δ − ✻ R.A. m s	Corr. for Parallax in R.A. s	δ − ✻ N.P.D. ′ ″	Corr. for Parallax in N.P.D. ″	No. of Comp.	App. R.A. h m s	App. N.P.D. ° ′ ″	
5 14 11	M.	+0 42·44		+1 21·0		5	16 33 12·10	25 33 32·2	*a*
		−0 24·90		+8 26·8		5			*b*
26 9 55	M.	+0 15·31	+0·35	+1 20·1	−1·5	4			*c*
		−0 32·44	+0·35	+8 29·8	−1·5	4	16 25 47·29	30 5 54·8	*d*

Assumed Mean Places of the Comparison Stars.

Star.	Star's Name.	R.A. 1883·0. h m s	N.P.D. 1883·0. ° ′ ″	Authority.
a	Oeltz. Arg. (N) 16374	16 32 29·78	25 32 31·6	Oeltz. Arg. (N) 1842
b	Anonymous			
c	,,			
d	Oeltz. Arg. (N) 16270	16 26 20·33	29 57 42·7	Oeltz. Arg. (N) 1842

Sept. 5. The comet was a very diffused and exceedingly faint object, becoming more and more difficult as it got lower. The positions are therefore exceedingly rough.

The observations are not corrected for parallax. The effect of refraction is insensible.

Sept. 26. Comet fairly bright; round; no tail; central condensation. Two transits of the first and second edge of the comet were taken to determine the diameter. The mean of these transits gives for the apparent diameter 2′ 21″·0.

The star *c* was very faint.

The observations are corrected for parallax. The effect of refraction is insensible.

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Observations of Comet b, 1883 (*Pons—Brooks*), *with the East Equatorial. Aperture 6·7 inches.*

Greenwich Mean Solar Time. 1883, Oct.	Obs.	δ − ✱ R.A.	Corr. for Par. in R.A.	δ − ✱ N.P.D.	Corr. for Par. in N.P.D.	No. of Comp.	App. R.A.	App. N.P.D.	
d h m		m s	s	′ ″	″		h m s	° ′ ″	
21 11 9	A.D.	−0 46·39	+0·36	+ 4 28·6	−2·5	2	16 46 12·20	35 19 20·8	*e*
11 12		−3 45·14	+0·36	+ 7 6·2	−2·6	1	16 46 17·42	35 20 27·0	*f*
11 38		+2 14·35	+0·35	− 3 42·7	−2·8	3	16 46 16·77	35 19 52·1	*g*
29 6 34	A.D.	+1 26·66	+0·33	+ 3 31·6	−1·2	3	16 58 51·16	36 50 18·0	*h*
6 49		−2 13·91	+0·34	− 6 26·3	−1·4	2			*i*
7 21		−2 35·65	+0·35	+13 43·5	−1·7	2	16 58 43·20	36 49 55·1	*j*

Assumed Mean Places of the Comparison Stars.

Star.	Star's Name.	R.A. h m s	N.P.D. ° ′ ″	Authority.
e	Arg. Z + 54° − 1839	16 46 59·23	35 15 7·8	Bonn Obs., vol. v.
f	Arg. Z + 54° − 1846	16 50 3·18	35 13 36·8	,,
g	Oeltz. Arg. (N) 16534	16 44 3·07	35 23 49·9	Oeltz. Arg. (N) 1842
h	Arg. Z + 53° − 1910	16 57 25·10	36 47 1·5	Bonn Obs., vol. v.
i	Anonymous			
j	Oeltz. Arg. (N) 16790	17 1 19·44	36 36 27·1	Oeltz. Arg. (N) 1842

Oct. 21. Comet very faint and difficult to observe; has a stellar nucleus.

Stars *e* and *f* about the tenth magnitude; *c* about the eighth.

Oct. 29. Comet faint and difficult to observe.

The observations are corrected for parallax. The effect of refraction is insensible.

The initials A.D. and M. are those of Mr. Downing and Mr. Maunder.

Royal Observatory, Greenwich:
1883, *Nov.* 9.

Observations of the Variable Star R Carinæ from May 1880 *to Sept.* 1883. By John Tebbutt.

My attention was first drawn to this star in May 1880 by the remarks of Dr. Gould in the *Uranometria Argentina*, who was, I believe, the first to point out its variability. It is synonymous with the star numbered 3932 in Lacaille's Catalogue of 9766 stars. Perceiving that it was one of the most remarkable variables beneath the horizon of northern Observatories I at once resolved to follow its variations systematically. I have therefore recorded its magnitude at every suitable opportunity. During the visibility of the star to the unassisted eye my comparisons were made with a selection of neighbouring stars from the *Uranometria*; when the variable became telescopic they were

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made with certain stars comprised with it in the same low-power field of my telescopes of $3\frac{1}{4}$ and $4\frac{1}{2}$-in. aperture. In order that the precise value of my estimates near the minimum may be understood by any future astronomer who may continue the observations of this star, I have appended a list of the small comparison stars, giving their assumed magnitudes and their approximate coordinates in R.A. and N.P.D., with reference to the variable itself. I may state that the smallest stars in the list—namely, those of the 9·2 magnitude—can just be seen with the aperture of the $4\frac{1}{2}$-in. telescope reduced to one inch. They are also found to be about equal to the average stars of the ninth magnitude in *Oeltzen-Argelander's Zonen Beobachtungen*, 15° *to* 31° *Sud. Dec. für* 1850. The brightest comparison in the field is Lacaille 3993, whose magnitude 6·9 is adopted from the *Uranometria*. The variable is red through all its stages, and it will be seen that although the star does not vary much from the ninth magnitude at the minimum, the value of the maximum magnitude varies considerably. Of the four maxima observed here, that of December 1880 was much the brightest. The variations about the maximum appear to be more rapid than those about the minimum. The following table exhibits the concluded magnitudes of the variable during the period May 1880–Sept. 1883; and as they are the results of very careful comparison, I trust they will prove an acceptable contribution towards data for an investigation of the period and other elements of this remarkable star. I shall continue the observations, more especially about the epochs of maximum and minimum. Comparing my own estimates with those recorded for 1871–1874 in the *Uranometria*, I conclude that the probable period from maximum to maximum is about 313 days. This conclusion, it will be seen, agrees with that already published by me in the *Journal of the Royal Society of New South Wales* for 1881.

Windsor, N.S. Wales:
1883, *Sept.* 5.

Concluded Magnitudes of R Carinæ.

Date.	Mag.	Date.	Mag.	Date.	
1880, May 11	8·2	1880, Aug. 9	9·0	1880, Nov. 14	6·0
12	8·3	12	9·0	15	6·0
18	8·3	22	9·0	17	5·9
28	8·5	Sept. 1	8·8	20	5·7
June 9	8·9	18	7·5	24	5·4
28	9·0	30	7·0	30	5·3
July 5	9·2	Oct. 10	6·9	Dec. 1	5·2
12	9·1	26	6·7	4	5·1
20	9·0	27	6·7	10	4·6
26	9·1	30	6·7	12	4·4
Aug. 4	9·1	Nov. 1	6·6	14	4·4

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Date.	Mag.
1880, Dec. 16	4·3
18	4·4
19	4·4
22	4·5
25	4·5
26	4·7
30	4·9
1881, Jan. 1	4·9
5	4·9
6	4·9
8	5·0
15	5·2
19	5·4
26	6·1
29	6·0
Feb. 10	6·4
15	6·6
26	7·0
Mar. 1	7·0
7	7·5
14	8·0
18	8·2
24	8·4
30	8·6
Apr. 7	8·7
13	8·8
19	8·8
28	9·0
May 4	9·1
10	9·1
16	9·1
21	9·1
June 1	9·2
8	9·3
20	9·1
27	9·0
July 6	8·9
14	8·9
20	8·8
29	8·7

Date.	Mag.
1881, Aug. 11	8·2
19	8·2
30	8·1
Sept. 16	7·4
Oct. 8	5·7
11	5·5
21	5·4
27	5·4
Nov. 7	5·5
13	5·9
17	6·3
18	6·2
22	6·3
Dec. 3	6·9
10	7·0
19	7·6
1882, Jan. 2	8·3
9	8·5
18	8·6
23	8·7
31	8·9
Feb. 10	9·0
15	9·0
20	9·1
24	9·1
Mar. 6	9·2
10	9·2
17	9·2
22	9·2
27	9·3
Apr. 4	9·2
5	9·2
9	9·2
15	9·3
20	9·3
24	9·2
May 5	9·2
11	9·1
20	9·1
June 4	8·8

Date	Mag.
1882, June 14	8·6
24	8·3
July 29	7·2
Aug. 10	6·0
21	5·0
30	4·9
Sept. 2	4·9
6	4·9
13	5·1
23	5·4
Oct. 6	5·7
10	6·5
Nov. 21	7·6
Dec. 12	9·8
18	8·5
29	8·8
1883, Jan. 12	9·1
16	9·1
25	9·2
Feb. 7	9·3
13	9.2
26	9·2
Mar. 4	9·2
10	9·1
28	9·0
Apr. 1	9·0
27	8·4
June 14	6·0
26	5·5
July 3	5·0
7	5·1
8	5.0
12	5.0
17	5·1
20	5·2
24	5·2
27	5·2
29	5·2
Aug. 1	5·5
6	5.7
Sept. 3	6·9

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Coordinates in R.A. and N.P.D. of the Comparison Stars with reference to R. Carinæ, together with the Adopted Magnitudes of the Comparison Stars.

Comparison Star.	Adopted Magnitude.	R.A.	N.P.D.
		m s	′ ″
No. 1 (Red Star) ...	8·7	1 31·4 E.	26 28 S.
2	9·2	1 32·1	8 5 S.
3	8·6	1 52·0	20 24 N.
4	9·2	2 56·1	0 45 S.
5	8·5	3 8·4	20 13 N.
6	9·2	3 52·6	10 8 S.
7	9·2	4 30·0	9 0 N.
8	8·0	6 4·9	14 41 N.
9	9·1	6 46·2	3 52 S.
10	8·0	6 46·8	14 51 N.
11	8·5	7 8·7	27 17 S.
12 (Lacaille 3993) ...	6·9	7 23·5	8 31 S.
13	8·9	8 2·2	12 9 S.
14	8·8	9 34·7	6 54 N.
15	8·6	10 8 0	3 33 N.

Note.—The position of star No. 7 is derived from alignments with the other stars.

Errata in my communications in vol. xliii. of the Monthly Notices of the Royal Astronomical Society.

Page 279. *For* 0″·7 *read* 0″·1.
„ 386. R.A. parallax factor for Oct. 26; *for* −8·7083 *read* −8·7089.
„ 387. Comparison Star for Nov. 14; *for* 31 *read* 35.
„ 389. Comet's App. N.P.D. for Dec. 29; *for* 119° 51′ 19″ *read* 119° 51′ 9·1″.
„ 393. Opposite to Star No. 54; *for* 1880, 3240 *read* 1860, 3240.

Measures of Southern Double Stars. By E. B. Powell.

In the *Monthly Notices* for June 1870, in which appeared my last published elements of the orbit of *α Centauri*, I mentioned my intention of communicating at a later period the details of the double star observations I was then engaged in taking. Circumstances prevented me from carrying out the plan I had formed of securing a considerable number of measures of different southern binaries; and, on my return to Europe in 1875, I thought it scarcely worth while to trouble the Society with the limited number of observations I had taken. As, however. Dr. Elkin applied to me some time ago for my unpublished

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detailed observations of *α Centauri*, it occurs to me that I may as well enable the Society to place them on record, should it appear advisable to do so; I therefore do myself the pleasure of forwarding them. Along with the measures of *α Centauri* are three other southern stars, and a few observations of *η Cassiopeiæ*. The measures now placed before the Society embrace all my Madras results subsequent to those recorded in vol. xxxii. of the *Memoirs*.

Bath, Sept. 24, 1883.

Star.	Pos.	No. Obs.	Dist.	No. Obs.	Epoch.
η Cassiopeiæ	122°·7	10	6″·42	4	1863·868
	122·8	6			1863·903
	123·6	6	6·63	6	1864·109
	123·6	8	6·78	10	1864·125
Mean	123·1	30	6·65	20	1864·001
	133·15	5			1870·058
	134·17	7	6·28	8	1870·066
	133·4	5	6·3	10	1870·071
	133·45	7	6·5	8	1870·074
	133·1	8	6·45	8	1870·079
Mean	133·4	32	6·39	34	1870·07
	137·6	6	5·9	6	1871·077
	136·96	8			1871·123
Mean	137·4	14	5·9	6	1871·1
p (6) Eridani	250 8	10	5·17	10	1863·004
	251·3	6	4·77	10	1863·015
	250·9	5	4·88	10	1863·02
	250·7	9	4·61	8	1863·029
Mean	250·9	30	4·9	38	1863·017

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Star.	Pos.	No. Obs.	Dist.	No. Obs.	Epoch.	
	°		"			
p (6) Eridani	243·0	6			1870·048	
	243·3	7			1870·063	
	243·2	3	5·55	10	1870·066	
	243·0	7			1870·071	
	242·7	6			1870·088	
	242·8	5	5·74	10	1870·099	
Mean	243·0	34	5·7	20	1870·072	
	241·4	5	5·6	6	1871·005	
	241·4	12	5·2	8	1871·077	
	241·7	10	5·4	10	1871·088	
	241·6	6			1871·09	
	241·5	7	5·8	8	1871·099	
	241·9	5	5·37	8	1871·104	
Mean	241·6	45	5·46	40	1871·077	
γ Centauri	6·2	5			1870·187	Distance about 1½" or 1½" by estimation. Distance measures beyond telescope.
	7·4	6			1870·198	
	8·0	5			1870·244	
	6·6	5			1870·252	
	7·65	4			1870·255	
	6·75	8			1870·26	
Mean	6·85	33			1870·233	
	8·1	6			1870·971	
	7·03	6			1871·061	
	6·7	7			1871·073	
	5·7	10			1871·092	
	5·85	7			1871·097	
Mean	6·16	36			1871·059	

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Star.	Pos.	No. Obs.	Dist.	No. Obs.	Epoch.	
	°	′	″			
α Centauri	1·2	10	7·35	10	1863·004	
	·83	12	7·4	10	1863·009	
	2·02	6			1863·024	
	1·3	6	7·4	6	1863·028	
	·9	10	7·04	10	1863·045	
	2·2	15			1863·056	3½ wrong; −½ much worse.
Mean	1·4	59	7·2	36	1863·028	
	5·9	10	7·67	8	1864·002	
	5·6	10			1864·094	
	5·6	8	7·9	8	1864·116	
	5·8	10	7·7	26	1864·121	
	5·6	6			1864·132	
	5·4	10	8·02	10	1864·152	7½ wrong, but not very bad; 3½ much worse.
	5·9	10	7·9	18	1864·156	
Mean	5·7	64	7·85	70	1864·11	
	11·18	5	8·87	6	1866·056	
	11·2	10	9·5	6	1866·061	
	11·0	9	9·37	10	1866·072	
Mean	11·1	24	9·3	22	1866·063	
	17·9	8	10·6	8	1869·127	
	18·0	14	9·9*	12	1869·138	
Mean	17·97	22	10·4	20	1869·132	

* Distance clipped as much as the eye could bear without being offended. Intended to try on another night how wide a distance the eye could bear, but did not carry out intention. Gave second night only ½ the weight of first night.

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Star.	Pos.	No. Obs.	Dist.	No. Obs.	Epoch.	
	°		″			
α Centauri	21·23	5	10·2	6	1870·012	
	20·7	10	10·29	10	1870·015	
	21·0	5	9·97	10	1870·018	Stars unsteady.
	20·43	15	10·25	32	1870·059	
	19·75	5	10·16	10	1870·067	Stars unsteady.
	20·28	8			1870·07	
	20·23	5	9·99	10	1870·073	Stars at times flaring.
	19·72	10	10·26	10	1870·081	
	20·28	5	10·46	20	1870·083	Stars beautiful.
	20·65	8	10·11	10	1870·187	
	20·27	10	10·38	20	1870·193	
	20·53	7	10·28	14	1870·198	
	21·27	7	10·08	10	1870·242	
Mean	20·45	100	10·24	162	1870·1	
	22·07	5			1870·591	
	21·75	5	9·88	8	1870·599	
	21·83	7	10·07	10	1870·602	
	22·12	5	10·16	10	1870·611	
	21·5	7	10·27	10	1870·635	Daylight.
Mean	21·8	29	10·09	38	1873·608	
	22·98	5	9·9	10	1870·971	
	22·9	5	9·8	10	1870·981	
	23·1	5	9·8	6	1870·984	Stars unsteady. Daylight.
	23·25	6	9·96	18	1871·061	Some night, some day.
	23·29	18	9·77	20	1871·073	Some night, some day. Some E., some W.
	23·33	7	10·0	10	1871·078	
	22·8	20	9·9	20	1871·092	
	22·7	7	9·93	20	1871·094	Daylight.
	23·02	7	10·07	10	1871·097	East.
	22·95	7	9·8	18	1871·102	West.
	23·03	5	9·92	20	1871·105	West.
Mean	23·01	90	9·89	162	1871·05	

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Star.	Pos.	No. Obs.	Dist.	No. Obs.	Epoch.	
	°		″			
α Centauri	23·0	7	9·8	10	1871·215	
	22·7	8	9·83	10	1871·22	
	23·2	5	10·0	6	1871·307	
	24·3	5	9·8	10	1871·32	
	23·8	7	9·9	20	1871·326	Some E., others W.
	24·2	8	9·66	10	1871·389	
	24·9	5	9·9	20	1871·391	
Mean	23·7	45	9·8	86	1871·31	
γ Coronæ Aust.	318·5	6			1863·775	
	317·6	12			1863·849	
	318·8	7			1863·857	
	318·4	5			1863·865	
Mean	318·1	30			1863·836	
	285·5	5			1870·187	Taken with a lower power than usual, and therefore perhaps subject to extra error.
	287·6	7			1870·193	
Mean	286·9	12			1870·19	
	281·9	12			1871·22	

On Humidity as a Cause of Variation of Rate in Chronometers.

By Major-General J. F. Tennant, R.E., F.R.S.

About the end of March, 1882, I borrowed from the Government stores a chronometer by Thomas Fletcher of London, No. 2684; it had been some time in India, but had not been cleaned since its arrival, and was said to have a good rate. When it came to me the rate was about $-6^{s}\cdot5$ (gaining), and this it seemed to keep fairly till the end of May (149^{d}). (I shall use the days of the year frequently, as I can readily take them from my graphic projection of data.) Then the rate suddenly fell to $-2^{s}\cdot0$ between 150^{d} and 170^{d}, rose nearly a second for a week, and then with small variations continued to fall till about 270^{d}, when it became insensible. At 290^{d} it rose a little, but shortly after I left Calcutta for a short holiday, and missed a period

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which would have been interesting. Of course, the watch ran down, and when I returned and started it again, after some unsteadiness, the rate was $-2\frac{1}{4}^s$ from 310^d to 316^d, and it rose to $-8^s{\cdot}0$ about 330^d to 350^d on the average. It again accidentally ran down, but was started towards the end of the year, and in the beginning of 1883 the rate was $-7^s{\cdot}8$, and it gradually rose numerically till in the period 59^d–65^d it was $-9^s{\cdot}2$. From 65^d–74^d the rate fell to $-6^s{\cdot}8$, and, after a slight rise, it gradually fell from 85^d till about 180^d it became $+0^s{\cdot}2$, and the change went on till, about the beginning of the present month (245^d), it was $-2^s{\cdot}6$.

I have compared these rates with a plot of the published daily mean temperatures at the Meteorological Observatory. Of course that is not quite satisfactory; but though at first sight it would seem that the rate depends on temperature, farther examination shows that it can only do so to a small extent, and confirms the belief I had from general impressions, and that which was formed by others who used it, that the rate does not depend on temperature. Except occasionally, when carefully carried by myself to a place where I could observe the Port Time Ball, which has been used for determining errors, the chronometer has stood in a room sheltered from sun, but always open to the air more or less. It is, however, certainly cooler in the hot season, and probably warmer in the cold season, than the outer air, and with a far more equable temperature. It has stood, too, in its outer case.

There was a cold period in 1882, about 130^d, when the mean of some days of temperature was about 77° F., and the rate was $-6^s{\cdot}7$. It was the same at 120^d, when the average was about 85° F., and about 140^d, when it was $-6^s{\cdot}0$. On the other hand, about 270^d, when the rate was $0^s{\cdot}0$, the temperature was about 83°·5 F. About 1883, 30^d, the temperature averaged barely 60°, while the rate was about $-8^s{\cdot}0$ on the average. When the rate was $-9^s{\cdot}2$ the temperature was about 78° F., whereas recently, when the rate has been losing rather than gaining, the temperature is about 82° F.

But there is no doubt that there is, however, a periodic change of rate, and I believe the cause to be *humidity*. My first suspicion was raised by the sudden fall of rate this year being coincident with the first heavy rains producing great damp, and the fact that it was so last year, and that the whole period of low gaining-rate was that of the rains, while the lowest was the warm time at the end of the rains, when the soil is loaded generally with moisture. The same phenomenon has recurred. It is, however, much more difficult to compare the supposed cause and effect without special arrangements. In my room the damp rises largely from the ground, and after very heavy rain the floor is discoloured, or even glistening with moisture in the early morning. The humidity of the air taken at the Observatory is thus no real measure of that of the room, but in any case

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it is doubtful whether air-humidity could be more than a rough guide.

If, as I suspect in this case, the oil on the arbours of the balance be hygroscopic, it is easy to see that it may become more fluid in damp weather, the arc of oscillation will increase, the balance vibration take longer, and the watch lose; but the momentary humidity of the air will not correspond to the rate as the temperature does more or less. Such a general accordance as I have shown seems the only possible result of comparison. But there seems in this case to me an undoubted connection, and it may be worth making special experiments in the following directions:

1st. Are chronometer oils, or any of them, hygroscopic?

2nd. Can they become so by exposure to a tropical climate? And in this last case I conceive that the climatic influence cannot be imitated in Europe. The effect of the heat, and probably the light, are very destructive of some materials. For instance, vulcanised indiarubber will not bear exposure in India, though it seems to answer in Europe even in heat and damp.

Lastly, in estimating the effect of humidity on a given chronometer, it will probably be best to use one of the old hair or grass hygroscopes for the humidity, placing it in the case enclosed with the watch.

P.S. As I shall be leaving India soon I shall not be in a position to investigate any farther with this watch.

H. M.'s Mint, Calcutta:
1883, *September* 14.

Showers of Large Meteors. The Muscids (Oct. 17–22) and Herculids (Oct. 17–26). By W. F. Denning.

A large meteor seen here on October 17, 1883, at $9^h\ 33^m$, as it traversed a nearly vertical path of 28° amongst the stars of *Hercules*, evidently belonged to a radiant point between *Aries* and *Taurus*, forming an early member of the prominent shower of Muscids (Oct. 25–Nov. 6), to which I called special attention in the *Monthly Notices*, vol. xlii. pp. 34–7. I had no idea, however, that this shower occurred with considerable brightness simultaneously with the Orionids, but on projecting the catalogued paths of a great many fireballs observed during the October meteoric epoch (Oct. 17–22) I found that the principal radiant lies at a very definite point S. of *Musca*, in the E. region of *Aries*, and close to the diverging focus of the later shower above referred to. In fact, this particular stream apparently gives many meteors of the finest class from about the middle of October

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to the end of the first week in November, and like its companion, though somewhat later, shower the Taurids, is of long continuance.

The following afford examples of these early and often brilliant Muscids:—

Year 1800+ and Date.			Mag.	Observed Path From α	From δ	To α	To δ	Length.	Observer.
49	Oct.	17	Fireball.	76°	+55°	188°	+41°	68	Heis, Aachen.
54		21	♀	72	+14	83	+12	11	Heis, Münster.
70		20	♃	10	+44	314	+53	38	Jadanza, Napoli.
70		22	♃	85	−11	90	−14	6	Maggi, Volpeglino.
72		19	♀	157·5	+71·3	183·3	+61·2	15	Holetschek, Wien.
77		20	♃	336	+22·5	293	+15·5	41	Denning, Bristol.
78		21	♀	5	−15	343	−32	27	Sawyer, Camb. U.S.A.
78		22	♃	3	−10	346	−28	25	„
83		17	♀	259	+48·5	241·5	+24	28	Denning, Bristol.

Radiant point at 40° + 20°.

A careful search through all the published catalogues of observers would doubtless reveal many other brilliant meteors belonging to this system.

The shower seems to have been especially active in 1871. Prof. Herschel at Hawkhurst* saw six meteors (on Oct. 14, 11^h to 12^h), "as bright as or brighter than first mag. stars, radiating with considerable accuracy from a point near the head of *Aries*, and close to the point of first appearance on this date of the radiant R_3 in *Musca* which appears to contribute bright meteors from the direction of this constellation during the principal meteor-showers of October, November, and December, but from which so many bright meteors in one hour as those seen at Hawkhurst on the above date form an exceptional display. Another meteor like one noted on this date, as bright as *Sirius*, proceeded from the same radiant point, passing overhead at Hawkhurst, and leaving a faint streak at $11^h\ 45^m$ P.M. on Oct. 19, and two scarcely less brilliant members of the same meteor-shower appeared with short courses and slow motion near the radiant point on Oct. 21." The great meteor of Oct. 19, 1877, $6^h\ 13^m$, may also have belonged to this stream, as the radiant point roughly inferred from a few uncertain observations was at 20° + 15°. The region of *Aries* is very rich in showers during October, and there is considerable difficulty in tracing their individual characters. In 1877, Oct. 28, Nov. 13, I saw a dis-

* B.A. Report on Meteors, 1872, p. 92.

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play of eleven slow meteors from the point 30°+16°, and in 1879 Oct. 14–20 recorded thirty-seven slow and generally small meteors from a radiant at 31°+9°, or 13° S. of *α Arietis*. Mr. Corder has also observed * many of the shooting stars of the latter shower, which also have been seen by Col. Tupman, notably in 1869, Oct. 13,† from the point 28°+10°. But these showers are probably quite distinct from the Muscids, relatively to which they supply meteors of far inferior magnitude.

I noticed this shower as early as Oct. 8, in 1877, when it furnished a display of eight short, quick meteors. On Oct. 12, 1868, it was splendidly seen by Sig. Zezioli at Bergamo. Twenty-two meteors (six per hour) were traced to a radiant point well-defined to within 1° or 2°, at 53°+27° (Schiaparelli). The latter position, though several degrees E. of the average centre of this shower, undoubtedly represents its very active return in that year. It evidently varies to a considerable degree in different years; for though at about the middle of October 1879 a large number of meteors were registered at Bristol, this radiant point gave no visible indication of activity.

It seems desirable to summarise the various determinations of the radiant point of this special meteor-stream.

Year.	Epoch.	Radiant. α δ	No. of Meteors.	Observer or Authority.
	Oct. 14–Nov.	46° + 27°		Greg and Herschel.
1868	12	53 + 27	22	Schiaparelli and Zezioli.
1872	29–Nov. 13	45 + 26	7	D. (from Italian obs.)
1876	14–15	46 + 26	13	D.
1876–9	Oct.–Nov.	43 + 15	34	Corder.
	„	41 + 13	13	„
1877	Oct. 8	47 + 28	8	D.
1877	28–Nov. 13	43 + 22	31	D.
1878	21–22	47 + 27	4	Sawyer.
1879	19	44 + 18	4	„
	20–31	40 + 21	18	D. (from Foreign obs.)
1879	Nov. 12–14	46 + 21	19	D.
1880	28	40 + 21	8	Weiss.
	Oct. 17–22	40 + 20	9	D. (rad. of fireballs).
	25–Nov. 6	48 + 22	17	„

Mean of 15 radiants = 44°·6 + 22°·3, Oct. 8–Nov. 28.

* *Monthly Notices*, vol. xl. p. 137. Radiants, Nos. 71, 72, and 73.
† See No. 93 of his Catalogue of Radiant Points.

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My recent investigation of the bright meteors of Oct. 17–22 enabled me to identify many fine Orionids from their recorded paths, but the radiant point of this well-known periodical shower is already sufficiently determined. I also found several bright Gemellids amongst the observations, indicating a radiant at 102°+26°.

The shower in *Hercules* occurring also at the October epoch is, however, comparatively unknown. It is situated at 247°+41°, near *η Herculis*, some 64° N.E. of the Sun's place, and in a region almost devoid of meteoric radiants at this time of the year. My attention was first attracted to this shower in 1876, but I had little more than a suspicion of its existence. I saw nothing of it in the several succeeding years, my observations being invariably directed to the eastern sky where, had any Herculids appeared, they would probably escape identification owing to their great distance from the radiant point.

But on Oct. 26, 1883, at $9^h\ 17^m$, while walking along a dark lane in a northern suburb of this city, I suddenly became aware of a brilliant flash illuminating objects around with startling distinctness, and followed by a more prolonged outburst of light. Immediately looking upwards I saw the nucleus of a large fireball passing close to *α Cephei* near the zenith. As I watched it disappear, it gave two other flashes, and then died out suddenly, leaving a broken streak, the brilliant sections of which marked the points where the succession of outbursts had occurred. The meteor rose upwards, traversing a path, as I roughly estimated it, of 24°, and its motion towards the end consisted of a series of spurts, accompanied with brilliant discharges. The streak became extinct in about four seconds, and soon afterwards the sky was covered with clouds, so that I found it impracticable to maintain a watch for further meteors of the same stream, the radiant point of which must evidently have occupied a position in the W.N.W. sky amongst the stars of *Hercules*.*

On consulting Heis's *Resultate 43 Jahren 1833–1875, Angestellten Sternschuppen-Beobachtungen*, I find he gives a radiant at

* Several other fireballs appear to have been noticed by different observers on the same date. Mr. D. Cameron says (*Nature*, Nov. 15, p. 56), "At $2^h\ 50^m$ A.M. on Oct. 26, a large ball of fire, apparently seven inches in diameter, illumined the heavens with great brilliancy as it descended from about midway between the third and fourth stars of *Auriga*." The same journal (Nov. 8, p. 44) states that "on Oct. 26, at about 7 P.M., a splendid meteor was seen in the district of Hernosand, Sweden. A traveller on the road to Ragunda says that he suddenly saw the night lit up as in broad daylight, which was caused by a large meteor appearing, with a blinding white lustre, in the zenith, and travelling very rapidly down to the horizon. When halfway, as it appeared to the observer, between the zenith and the Earth, it suddenly burst, throwing a quantity of sparks in every direction." The extremely vague character of these descriptions renders them almost useless, but the observations are worth referring to as showing the prevalence of fireballs on Oct. 26 of this year. It is much to be regretted that in such cases observers generally omit to give those most essential details—the direction and position of the apparent paths.

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238°+51° from 19 meteors seen between Oct. 17 and Nov. 14, and this shower from the extreme N.W. region of *Hercules* is probably the same as that from which the recent fireball took its departure. Prof. N. de Konkoly had also suspected the shower from observations at O'Gyalla on Oct. 22, 1872, and derived the centre at 243°+42°.* The radiant is well supported by several brilliant meteors recorded at the same station on Oct. 22, 1873, though this corroboration does not appear to have been previously mentioned. It is obvious that this particular stream is one which merits close watching in future years, not only because its gives a profusion of brilliant meteors, but also on account of the comparatively isolated situation of the radiant point. Owing to the latter circumstance this shower, if a prominent one, will admit of very ready determination, as its meteors will show a peculiarity of direction as compared with those proceeding from the large number of radiants clustering in the eastern sky. It will be best observed in the early evening hours, before the radiant falls low on the northern horizon.

The following are a few bright meteors belonging to this system of Herculids (Heis F_9):

Year 1800+ and Date.			Mag.	Observed Path From α	From δ	To α	To δ	Length.	Observer.
53	Oct.	23	1	8°	+34°	18°	+26°	12	Heis, Münster.
60		17	4	237	+32	229	+23	11½	„
63		25	♀	41	+41	43	+31	10	„
63		25	♀	41	+48	44	+39	9	„
73		22	1	321·4	+ 7·0	352·6	−16·6	39	Konkoly, O'Gyalla.
73		22	♀	330·4	+18·5	5·8	−10·8	46	„
83		26	?=½)	288	+56	333	+59	24	Denning, Bristol.

Radiant diffuse near η *Herculis*, 247°+41°.

The above seems to have furnished large meteors at intervals of about ten years. All those observed appear to have left bright streaks. The reappearance of this shower should be carefully looked for in future years. It is true that at this epoch the Orionids usually monopolise attention, but the radiant in *Orion* does not rise until nearly 9 P.M., so that during the three hours preceding this the observer may be usefully employed in watching the N.W. sky for some indication of the return of this apparently rich shower in *Hercules*.

Bristol: 1883, *Nov.* 8.

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* *Beobachtungen angestellt am Astrophysicalischen Observatorium in O'Gyalla* 1879, p. 70.

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Ephemeris for finding the Positions of the Satellites of Mars, 1883–84. By A. Marth.

The major and minor semi-axes a and b of the apparent ellipses described by the satellites, the longitudes $u - U$ of the satellites reckoned in their orbits from the points which are in superior conjunction with the centre of the planet, and the angle of position P of the minor axes of the apparent orbits are approximately the following :—

		Phobos.					*Deimos.*			
Greenw. Noon. 1883.		a_1	b_1	$u_1 - U$	Diff. 2250° +	P	a_2	b_2	$u_2 - U$	Diff. 570° +
		″	″	°	°	°	″	″	″	°
Dec.	19	15·38	+4·93	36·05	7·68	4·56	38·42	+12·32	271·99	0·25
	21	15·64	5·00	133·73	·73	4 62	39·06	12·49	122·24	·30
	23	15·90	5·06	231·46	·78	4·65	39·71	12 65	332·54	·36
	25	16·16	5·12	329·24	·84	4·65	40·36	12·79	182·90	·41
	27	16·42	5·18	67·08	·89	4·61	41·01	12·93	33·31	·47
	29	16·68	5·23	164·97	·93	4·54	41·65	13·06	243 78	·52
	31	16·93	5·27	262·90	7·99	4·43	42·29	13·17	94·30	·57
1884. Jan.	2	17·18	+5·31	0·89	8·05	4·29	42·92	+13·26	304·87	0·63
	4	17·43	5·34	98·94	·09	4·12	43·54	13·34	155·50	·68
	6	17·67	5·36	197·03	·14	3·91	44·14	13 40	6·18	·74
	8	17·90	5·38	295·17	·19	3·67	44·72	13·44	216·92	·78
	10	18·12	5·39	33·36	·24	3·40	45·27	13·47	67·70	·83
	12	18·33	+5·39	131 60	8·28	3·09	45·79	+13·47	278·53	·88
	14	18·53	5·38	229·88	·32	2·75	46·28	13·45	129·41	·93
	16	18·71	5·37	328·20	·35	2·39	46·73	13·40	340·34	0·96
	18	18·87	5·34	66·55	·39	2·00	47·13	13·33	191·30	1·00
	20	19 01	5·30	164·94	·41	1·59	47·48	13·24	42·30	·03
	22	19·13	+5·26	263·35	8·44	1·15	47·77	+13·13	253·33	·06
	24	19·22	5·20	1·79	·45	0·69	48·01	12·99	104·39	·08
	26	19·29	5·14	100·24	·47	0·21	48·19	12·83	315·47	·10
	28	19·34	5·07	198·71	·47	359 73	48·31	12·66	166·57	·10
	30	19·36	4·99	297·18	·47	359·23	48·36	12·46	17·67	·11
Feb.	1	19·35	+4·90	35·65	·46	358·73	48·34	+12·25	228·78	·10
	3	19·32	4·81	134·11	·45	358·23	48·26	12·02	79·88	·10
	5	19·26	4·72	232·56	·43	357·74	48·11	11·79	290·98	·08
	7	19·18	4·62	330·99	·40	357·25	47·90	11·55	141·06	·06
	9	19·07	4·52	69·39	·37	356·77	47·63	11·30	353·12	·04

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Greenw. Noon. 1884.	*Phobos.* a_1	b_1	u_1-U	Diff. 2230° +	P	*Deimos.* a_2	b_2	u_2-U	Diff. 570° +
	″	″	°	°	°	″	″	°	°
Feb. 11	18·94	+4·42	167·76	8·34	356·31	47·30	+11·05	204·16	1·00
13	18·78	4·32	266·10	·30	355·86	46·92	10·80	55·16	0·97
15	18·61	4·22	4·40	·26	355·44	46.49	10·56	266·13	·93
17	18·42	4·13	102·66	·21	355·04	46·01	10·32	117·06	·89
19	18·21	4·04	200·87	·17	354·67	45·49	10·09	327·95	·85
21	17·99	+3·95	299·04	8·12	354 32	44·94	+ 9·86	178·80	·80
23	17·76	3·86	37·16	·07	354·00	44·36	9·64	29·60	·76
25	17·51	3·78	135·23	8·01	353·71	43·75	9·44	240·36	·71
27	17·26	3 70	233·24	7·96	353·45	43·12	9·24	91·07	0·65
29	17·00	3·63	331·20	·91	353·23	42·47	9·06	301·72	·60
Mar. 2	16·74	+3·56	69·11	7·85	353·04	41·81	+ 8 90	151·32	·55
4	16 47	3·50	166·96	·80	352·88	41·14	8·74	2·87	·50
6	16·20	3·44	264·76	·75	352·76	40·47	8·60	213·37	·45
8	15·93	3·39	2·51	·70	352·67	39·79	8·47	63·82	·40
10	15·66	3·34	100·21	7·64	352·61	39·12	8·35	274·22	0·36
12	15·39	+3·30	197·85		352·58	38·45	+ 8·25	124·58	

These values are to be interpolated for the times for which the positions of the satellites are required, the equation of time having already been taken into account. The position-angles p and distances s are then to be found by means of the formulæ :—

$$s \sin (p-P) = a \sin (u-U)$$
$$s \cos (p-P) = b \cos (u-U)$$

The satellites move in the direction of *in*creasing position-angles, and will be at their greatest eastern and western elongations (e in pos. P+90° and w in pos. P−90°) about the following hours, Greenwich Mean Time :—

1883.	*Phobos.* h	h	*Deimos.* h	1883.	*Phobos.* h	h	*Deimos.* h
Dec. 19	12·6 w	16·5 e	15·0 e	Dec. 24	11·3 e	15·1 w	16·2 e
20	11·6 w	15·4 e	21·3 e	25	10·2 e	14·0 w	7·3 w
21	10·6 w	14·4 e	12·4 w	26	9·2 e	13·0 w	13·6 w
22	9·5 w	13·3 e	18·7 w	27	12·0 w	15·8 e	19·9 w
23	12·3 e	16·1 w	9·9 e	28	10·9 w	14·7 e	11·1 e

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		Phobos.		*Deimos.*
1883.		h	h	h
Dec.	29	9·9 *w*	13·7 *e*	17·3 *e*
	30	12·7 *e*	16·5 *w*	8·5 *w*
	31	11·6 *e*	15·5 *w*	14·8 *w*
1884.				
Jan.	1	10·6 *e*	14·4 *w*	5·9 *e*
	2	9·5 *e*	13·4 *w*	12·2 *e*
	3	12·3 *w*	16·2 *e*	18·5 *e*
	4	11·3 *w*	15·1 *e*	9·6 *w*
	5	10·2 *w*	14·1 *e*	15·9 *w*
	6	9·2 *w*	13·0 *e*	7·0 *e*
	7	12·0 *e*	15·8 *w*	13·3 *e*
	8	10·9 *e*	14·8 *w*	19·6 *e*
	9	9·9 *e*	13·7 *w*	10·7 *w*
	10	8·9 *e*	12·7 *w*	17·0 *w*
	11	11·6 *w*	15·5 *e*	8·1 *e*
	12	10·6 *w*	14·4 *e*	14·4 *e*
	13	9·5 *w*	13·4 *e*	5·6 *w*
	14	8·5 *w*	12·3 *e*	11·8 *w*
	15	11·3 *e*	15·1 *w*	18·1 *w*
	16	10·2 *e*	14·1 *w*	9·2 *e*
	17	9·2 *e*	13·0 *w*	15·5 *e*
	18	12·0 *w*	15·8 *e*	6·2 *w*
	19	10·9 *w*	14·8 *e*	12·9 *w*
	20	9·9 *w*	13·7 *e*	19·1 *w*
	21	8·8 *w*	12·7 *e*	10·3 *e*
	22	11·6 *e*	15·4 *w*	16·5 *e*
	23	10·6 *e*	14·4 *w*	7·7 *w*
	24	9·5 *e*	13·4 *w*	13·9 *w*
	25	8·5 *e*	12·3 *w*	20·2 *w*
	26	11·3 *w*	15·1 *e*	11·3 *e*
	27	10·2 *w*	14·0 *e*	17·6 *e*
	28	9·2 *w*	13·0 *e*	8·7 *w*
	29	11·9 *e*	15·8 *w*	14·9 *w*
	30	10·9 *e*	14·7 *w*	6·1 *e*
	31	9·9 *e*	13·7 *w*	12·3 *e*
Feb.	1	8·8 *e*	12·6 *w*	18·6 *e*
	2	11·6 *w*	15·4 *e*	9·7 *w*
	3	10·5 *w*	14·4 *e*	16·0 *w*

		Phobos.		*Deimos.*
1884.		h	h	h
Feb.	4	9·5 *w*	13·3 *e*	7·1 *e*
	5	8·4 *w*	12·3 *e*	13·4 *e*
	6	11·2 *e*	15·1 *w*	19·6 *e*
	7	10·2 *e*	14·0 *w*	10·8 *w*
	8	9·1 *e*	13·0 *w*	17·0 *w*
	9	8·1 *e*	11·9 *w*	8·2 *e*
	10	10·9 *w*	14·7 *e*	14·4 *e*
	11	9·8 *w*	13·7 *e*	20·7 *e*
	12	8·8 *w*	12·6 *e*	11·8 *w*
	13	7·7 *w*	11·6 *e*	18·1 *w*
	14	10·5 *e*	14·3 *w*	9·2 *e*
	15	9·5 *e*	13·3 *w*	15·5 *e*
	16	8·4 *e*	12·3 *w*	6·6 *w*
	17	7·4 *e*	11·2 *w*	12·9 *w*
	18	10·2 *w*	14·0 *e*	19·1 *w*
	19	9·1 *w*	12·9 *e*	10·3 *e*
	20	8·1 *w*	11·9 *e*	16·5 *e*
	21	7·0 *w*	10·9 *e*	7·7 *w*
	22	9·8 *e*	13·6 *w*	13·9 *w*
	23	8·8 *e*	12·6 *w*	20·2 *w*
	24	7·7 *e*	11·6 *w*	11·4 *e*
	25	6·7 *e*	10·5 *w*	17·6 *e*
	26	9·5 *w*	13·3 *e*	8·8 *w*
	27	8·4 *w*	12·3 *e*	15·0 *w*
	28	7·4 *w*	11·2 *e*	6·2 *e*
	29	6·4 *w*	10·2 *e*	12·5 *e*
Mar.	1	9·1 *e*	13·0 *w*	18·7 *e*
	2	8·1 *e*	11·9 *w*	9·9 *w*
	3	7·1 *e*	10·9 *w*	16·2 *w*
	4	9·8 *w*	13·7 *e*	7·3 *e*
	5	8·8 *w*	12·6 *e*	13·6 *e*
	6	7·8 *w*	11·6 *e*	4·8 *w*
	7	6·7 *w*	10·6 *e*	11·1 *w*
	8	9·5 *e*	13·3 *w*	17·3 *w*
	9	8·5 *e*	12·3 *w*	8·5 *e*
	10	7·4 *e*	11·3 *w*	14·8 *e*
	11	6·4 *e*	10·2 *w*	5·9 *w*
	12	9·2 *w*	13·0 *e*	12·2 *w*

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In order to determine the longitudes of the satellites in their orbits properly it is essential that observations should be taken when the satellites are as far away from their greatest elongations as they can be seen.

In extending the ephemeris as far as it is given I have been guided by the circumstance that *Deimos* has been observed for position at the Harvard College Observatory on Jan. 10, 1880.

10.

Erratum.

Vol. XLIII., page 285, 1882, Oct. 24, *for* Reapp. 51 Piscium, *read* Disapp. 51 Piscium.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLIV. DECEMBER 14, 1883. No. 2.

E. J. STONE, M.A., F.R.S., President, in the Chair.

Charles Henry Clarke, B.A., The International College, Spring Grove, Isleworth;

Joseph Morrison, M.A., M.D., Office of the *American Ephemeris*, Washington, U.S.A.; and

Major C. Hawkins Fisher, The Castle, Stroud, Gloucestershire;

were balloted for and duly elected Fellows of the Society.

On the supposed possible Independence of Bessel's and Le Verrier's Expressions for the Mean Longitude of the Sun subject to the Conditions imposed upon them by their Use in practical Astronomy in the Determination of Mean Solar Time. By E. J. Stone, M.A., F.R.S.

The real point at issue between me and those who differ from my views with respect to the principal cause of the large existing discordances between Hansen's Tables of the Moon and observations, is as follows. Can we, consistently with the uses we have made of Bessel's and Le Verrier's expressions for the longitudes of the mean Sun, consider that there are two independent mean Suns without any necessary physical connection? That the question really resolves itself into this is easily shown. For let S denote the length of the sidereal day, and

$$T_1 = (1 + x_1)S, \quad T_2 = (1 + x_2)S,$$

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whatever values be assigned to x_1 or x_2. Then if ω_1, n_1, ω_2, n_2, are the angular velocities of the Earth about its axis, and the *true* Sun's mean motion in longitude in the units T_1 and T_2 respectively, we have identically

$$\frac{\omega_1}{\omega_2} = \frac{n_1}{n_2} = \frac{T_1}{T_2}.$$

This follows from the very definition of angular velocities and units of time, and therefore

$$\frac{T_1 - T_2}{T_2} = \frac{n_1 - n_2}{n_2}$$

which is my result; and one which must be true if n_1 and n_2 refer to any continuous motion.

But if we determine x_1 and x_2 so that

$$2\pi x_1 = \alpha_1, \quad 2\pi x_2 = \alpha_2,$$

α_1, α_2 being the circular measures of two angles; then we have, since 2π is the value of ω when S is the unit,

$$\omega_1 = 2\pi(1 + x_1) = 2\pi + \alpha_1$$
$$\omega_2 = 2\pi(1 + x_2) = 2\pi + \alpha_2.$$
$$\therefore \frac{\omega_1}{\omega_2} = \frac{T_1}{T_2} = \frac{2\pi + \alpha_1}{2\pi + \alpha_2}$$
$$\therefore \frac{T_1 - T_2}{T_2} = \frac{\alpha_1 - \alpha_2}{2\pi + \alpha_2} = \frac{\alpha_1 - \alpha_2}{\omega_2}$$
$$\therefore \frac{T_1 - T_2}{T_2} = \frac{\alpha_1 - \alpha_2}{\omega_2} = \frac{n_1 - n_2}{n_2}.$$

My result is therefore not inconsistent with, but identical with, that which has been previously obtained, for of course we can, if we please, assume

$$L_1 = A_1 + \alpha_1 t$$
$$L_2 = A_2 + \alpha_2 t,$$

and call L_1 and L_2 the longitudes of the mean Suns (1) and (2).

The real divergence between my views and those which have hitherto been accepted arises at this point; can we found our measures of mean solar time upon the expressions

$$L_1 = A_1 + \alpha_1 t, \text{ and } L_2 = A_2 + \alpha_2 t,$$

and change from the one to the other, when they are used, as we do use them, without breaks in the continuity of our measures of time? or without the changes in the units which my theory indicates?

If this interchange of expressions be possible without any

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breaks of continuity in our measures of time or without equivalent changes in all our *adopted mean motions of the Moon and planets*, we can assume without error

$$a_1 = \text{Bessel's adopted value},$$

$$a_2 = \text{Le Verrier's adopted value},$$

and still infer that

$$\frac{T_1 - T_2}{T_2} = \frac{a_1 - a_2}{\omega_2} = \frac{n_B - n_{LV}}{\omega_2};$$

but if the two mean Suns cannot, consistently with the use which we have made of them, be regarded as independent, then it is quite impossible to have both $a_1 = n_B$ and $a_2 = n_{LV}$, as the discordance between my result and that usually accepted shows immediately.

Now, a conditional relationship between the two Suns is at once established when a *third* real continuous motion (that of the Moon, for instance) is connected with the motions of the centre of gravity of the Earth and that of the rotation of the Earth about its centre of gravity and expressed in terms of t. So long as we are dealing with the *two* independent motions, that of the centre of gravity of the Earth and that about the centre of gravity of the Earth, there is no absolute necessity of separating the concrete factors ω and t, and n and t, in the discussion of the ratio of the angular velocity of the Earth to that of the Sun's mean motion in longitude. But this separation has to be made before we can discuss the Moon's motion, or, indeed, our own motion with reference to its position in space. This is the point which, I believe, has been overlooked by mathematicians. It is one which might certainly not present itself naturally to a mind occupied only with the Solar Theory, and it presented itself to my mind simply because I was examining the Solar Theory with direct reference to the bearings of questions of time upon the Lunar Theory. Mathematically expressed, what I have been stating amounts to this.

If the Sun's mean longitude

$$= \epsilon + nt, \qquad (1)$$

and we find from our investigations of the Lunar Perturbations that the Moon's longitude

$$= A + Nt + \text{Periodic terms}, \qquad (2)$$

then N and n are necessarily referred to the *same* unit of time, *whatever that unit may be*; and the angle n, assigned or supposed to be known, is such that in *each unit of time* the Sun's mean longitude increases n. But before our Lunar Theory can be compared with the observations, we require the theoretical expressions for the observer's position as well as those for the centre of gravity of the Moon. As a matter of fact we do not, in our theoretical investigations, introduce any such thing as a

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mean Sun as distinguished from the true Sun, and we employ the same numerical value n, which expresses the Sun's mean motion in longitude, to establish the relationship between the motion of the centre of gravity of the Earth and the motion about that centre of gravity. The distinction, therefore, between the longitude of any mean Sun and the mean longitude of the true Sun is one not recognised in our theoretical work; and as we use, not an imaginary Sun, but the real Sun for establishing the relationship between the motion of the centre of gravity of the Earth and the motion about the centre of gravity of the Earth, it follows that if we do determine our mean time by equating the motion of the meridian to a mean Sun, then that mean Sun must be the true Sun in order that the times thus determined may be those upon which our theories are constructed, and for which they alone can be true.

It has been usual to eliminate the angular velocity of the Earth about its axis, from our equations, by supposing

$$\omega = 360 \times 60 \times 60'' + n. \qquad (3)$$

But the equations (1), (2), and (3) cannot co-exist except for *one* definite and determinate angle n; but if T be the unit of time in which the Sun's mean longitude does increase n, and the angle through which the Earth has turned about its axis increases $360 \times 60 \times 60'' + n$, we can at once find the true corresponding equations when we adopt any other angles n_1 or n_2 instead of n for the Sun's mean motion in longitude. For if T_1 and T_2 are two times expressed in terms of T such that $nT_1 = n_1$ and $nT_2 = n_2$: then if we adopt T_1 and T_2 as our units of time the angular velocities of the Sun in mean longitude will be n_1 and n_2 respectively; and conversely, if we adopt n_1 and n_2 for the Sun's mean motion in longitude, then the units of time must, consistently with the truth of (1) and (2), be T_1 and T_2.

If, therefore, t_1 and t_2 denote the times, when n_1 and n_2 are adopted, which correspond to the same *absolute instant* as that indicated by t when n is adopted, we have

$$\text{Sun's mean longitude} = \epsilon + nt = \epsilon + n_1 t_1 = \epsilon + n_2 t_2$$

$$\text{Moon's mean longitude} = A + Nt = A + N\frac{n_1}{n} \cdot t_1 = A + N\frac{n_2}{n} \cdot t_2$$

and

$$\text{R.A. of meridian in arc} = \epsilon + \omega t = \epsilon + (360 \times 60 \times 60'' + n)t$$

$$= \epsilon + (360 \times 60 \times 60'' + n_1)t_1 + 360 \times 60 \times 60''\left(\frac{n_1 - n}{n}\right) \cdot t_1$$

$$= \epsilon + (360 \times 60 \times 60'' + n_2)t_2 + 360 \times 60 \times 60''\left(\frac{n_2 - n}{n}\right) \cdot t_2$$

Converting these expressions into time in the usual way we have

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$$\text{R.A. of meridian} = C + \left(24^h + \frac{n}{15}\right).t$$

$$= C + \left(24^h + \frac{n_1}{15}\right).t_1 + 24^h\left(\frac{n_1 - n}{n}\right).t_1$$

$$= C + \left(24^h + \frac{n_2}{15}\right).t_2 + 24^h\left(\frac{n_2 - n}{n}\right).t_2$$

Now, if n_1 and n_2 are the two values of the Sun's mean motion assumed by Bessel and Le Verrier, and B and V denote their adopted values of the sidereal times at corresponding mean noons, when $24^h.t_1 = 24^h \times$ some integer, and $24^h.t_2 = 24^h \times$ same integer, we shall have

$$\text{True R.A. of meridian at Bessel's mean noons} = B + 24^h\frac{n_1 - n}{n}.t_1$$

$$\text{,, ,, Le Verrier's mean noons} = V + 24^h\frac{n_2 - n}{n}.t_2$$

The difference, therefore, which arises from the use of Le Verrier's expression instead of Bessel's is

$$= V - B + 24^h.\frac{n_2 - n_1}{n}.t.$$

The term

$$24^h.\frac{n_2 - n_1}{n}.t$$

has been usually neglected on the impossible assumption that we could use either Bessel's or Le Verrier's expressions for the sidereal times at mean noon, without allowing for the change in the direction of the meridian between the two noons; and it has been the principal, if not the only argument against the acceptance of my views, that when you do neglect the terms

$$24^h.\frac{n_1 - n}{n}.t_1 \quad \text{and} \quad 24^h.\frac{n_2 - n}{n}.t_2$$

which I have proved to be necessary corrections to B and V before the true mean times can be correctly deduced from the sidereal times, the times thus incorrectly found only differ by the 366th part of the difference indicated by my theory. If we suppose Le Verrier's value n_2 to be the true value, a supposition which will give the true difference, we have when the times were determined by B the true time

$$t_1 + \delta t_1 = t_1 + \frac{n_2 - n_1}{n_2}.t_1,$$

and if we determine, therefore, the Moon's mean motion with these defective times t_1 we shall find instead of N, the value $N + \delta N$ such that

$$N(t_1 + \frac{n_2 - n_1}{n_2}.t_1) = (N + \delta N)t_1$$

or

$$\delta N = \frac{n_2 - n_1}{n_2}.N,$$

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and consequently, in 1864, when we replaced B by V, we ought to have changed the adopted value of the Moon's motion to the extent indicated; and all our other adopted mean motions should have been similarly changed to allow for the change in the adopted unit of time.

It is difficult to understand how any practical astronomer can seriously maintain that we are in these questions of mean solar time only concerned with the Sun's hour-angles, and not with the direction of the meridian in space. As a matter of fact our clock-errors are deduced from sidereal times which directly depend upon the position of the meridian with respect to the equinox.

The whole of our difficulties have arisen from attempts to carry on simultaneously an approximation to the Sun's mean motion in the true mean solar day, and to use that true mean solar day as our unit of time.

The equation $\omega T - nT = 2\pi$ has been theoretically treated as if the factor T, the adopted unit of time, did not exist, and as if the true equation was $\omega - n = 2\pi$. It has therefore been thought that the only possible way of keeping $\delta(\omega T) - \delta(nT) = 0$ was to take $\delta\omega = \delta n$, a condition which keeps the Sun's hour-angle unchanged, but throws the direction of the meridian of the observer out of its true position; such a solution cannot therefore be the true solution of our physical problem. Directly the unit T is introduced the difficulty disappears. All we require to keep the hour-angle unchanged is that

$$\delta\omega T + \omega\delta T = \delta n T + n\delta T,$$

which is satisfied if

$$-\frac{\delta T}{T} = \frac{\delta\omega}{\omega} = \frac{\delta n}{n},$$

and as these relations satisfy the necessary conditions that the Sun's mean longitude, and the direction of the meridian in space, should both be unaltered by the changes introduced into the adopted value of n, the solution thus obtained is the only possible solution which will bear comparison with the facts of observation.

Although it may appear useless to continue researches of which the fundamental principle is still disputed by astronomers of note, I may mention that, besides accounting for the missing $4''$ required to reconcile our observations with the theoretical value of the secular acceleration of the Moon's mean motion in longitude, I have convinced myself that the long inequality with a coefficient of $21''$ introduced by Hansen, but whose coefficient was found to be insensible by Delaunay, is, to very nearly its full extent at all events, an empirical inequality rendered necessary to reconcile theory and observation on account of breaks in the continuity of our measures of time similar to that which took place in 1864. I have not seen at present any evidence which weakens in the slightest degree my confidence in the accuracy of my results.

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On the supposed Unreliability of the Greenwich Observations of the Sun in Mean Results. By E. J. Stone, M.A., F.R.S.

In the *Monthly Notices* for November there appears a paper by my friend Mr. Dunkin on the "Greenwich Mean Tabular Errors of the R.A. of the Sun." The paper is an interesting one, as showing examples of considerable personalities between the different Greenwich observers. But the conclusion to which Mr. Dunkin is led that the apparent increase in the Tabular Errors in R.A. of the Sun since 1864 is due solely to these personalities, is one which appears to me unwarranted by the facts. The subject which Mr. Dunkin has attempted to treat is one of very great difficulty indeed.

With respect to personal errors which can be quantitatively determined directly, I am strongly of opinion that corrections should be applied to reduce each observer's work to the mean habit of all the regular observers; but with respect to personalities which can only be determined indirectly by assuming that the errors of the Tables disappear in the mean of a considerable number of results, I seriously doubt whether any great advantages are likely to result from their application.

But whatever may be the value of my opinion on the point, the usage at Greenwich has been to trust to mean results without any attempts, so far as I am aware, to correct for personalities except in the determination of clock-rate. The effect of this is to impress the mean habit of the general body of the observers upon the fundamental data; and consequently, although the Greenwich results are, in my opinion, thoroughly reliable and of great accuracy as mean results, the individual observations of a selected observer, instead of being free from personalities, as Mr. Dunkin seems to suppose, are generally more affected by such personalities than the general mean. This point has frequently been overlooked, but I believe that there is little doubt of its importance in the present case. The observation of the R.A. of the Sun consists of a difference between the times of transit of the centre, as determined from the times of transit of the limbs, and some one star corrected for clock-rate applied to some adopted Right Ascension of the star. In practice there will, of course, be usually several stars observed and the mean result taken. If, therefore, an observer has any distinct personality in such observations, this will prevent his so-called observed R.A. from agreeing with the tabular error given by the other observers, but the exact error which he gives will depend also upon the adopted Right Ascensions, and we can make it positive or negative, large or small, by adopting different values of the fundamental clock-stars.

Independently, therefore, of personal changes, there are to be considered the changes in the adopted clock-stars. Now, it has been the plan at Greenwich to discuss *all* the solar observations

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of each year, for the determination of the corrections required by the adopted clock-stars, in order to refer them to the true mean equinox of the year. Such corrections to epoch are necessarily affected by the general mean habit of the body of the observers employed on the observations. From time to time the stellar observations of several years are collected and reduced to some common epoch, and corrected for the mean of the corrections deduced for the several years from the solar observations. These Sun observations have therefore become fundamentally incorporated in the Greenwich work; and if untrustworthy in mean results, the consequences are serious indeed. Now, the observations 1864 to 1869 were all based fundamentally upon the Right Ascensions of the 1860 Catalogue, which embraces the observations made from 1854 to 1860, and is affected by the mean correction to epoch of the observers who made these observations.

The observations 1870–1877 are based fundamentally upon the Right Ascensions of the 1864 Catalogue, and are affected by the mean correction to epoch of the observers during the period 1860–1867; whilst the observations made 1878–1881 are based on the standard places of the Nine-Year Catalogue, which embraces the observations made 1868–1876, and are affected by the mean habit of the observers during that period. It is clear, therefore, that if there have been such serious changes as those which Mr. Dunkin supposes to have taken place in the mean habit of the general body of the observers, then Mr. Criswick's isolated observations should have shown changes in the tabular errors of the Sun during the period 1864–1881, unless his personal habit has also changed. On the other hand, if there have been no such great changes in the mean habit of all the observers, then the mean errors in longitude are trustworthy results. The only real test of any considerable change in the mean habits of the observers is to see whether there is, or is not, any great change in the correction to epoch when stars are brought up from one epoch to another with precession constants and proper motions which are supposed to be known with sufficient accuracy not to seriously affect the observed differences. If we apply this test to the observations 1864–1869 and 1878–1881, we find the mean correction to epoch for the first period is $-0^s{\cdot}020$; whilst that for the second period is $-0^s{\cdot}015$. The difference is only $0^s{\cdot}005$ or $0''{\cdot}075$; and it is clear that there is, so far as we can thus check the point, no indication of any serious change of mean habit, such as Mr. Dunkin assumes to have taken place.

The principle of separating the different observers' work without referring to the mean equinox of each observer is, I feel certain, radically unsound. So far as materials exist for the inquiry, it appears clear to me that Mr. Criswick's observations do show an increase of tabular error between 1866·5 and 1879·5 when reduced to his own epochs. It is probable, but not quite certain, that the observations 1878–1881 when referred to their own equinox should be increased by about $0''{\cdot}24$ relatively to

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those about 1864, on account of the addition now made to the reading for the line of collimation at Greenwich for effects of observations through the cubes.

So many changes were made during the period 1870 to 1877, and these changes have become so mixed up in the fundamental determinations, that it appears almost hopeless to attempt to disentangle their effects; but probably in mean results the errors are exceedingly small. It appears clear to me that unless we can trust the general mean results the Greenwich observations can afford us no reliable information on the question raised, and it is certainly illogical to assume that these Sun observations are sufficiently accurate to determine the corrections to epoch, and then afterwards to maintain that the mean results are unreliable.

The mean errors of the tables in longitude for the separate years, and the mean errors for the groups of years, 1864–1869, 1870–1877, 1878–1881, which result from the observations made by all the observers, are as follows:—

Year.	Mean Error.	No. of Obs.	Mean Error of Group.
	"		
1864	+0·025	122	
1865	+0·248	122	
1866	−0·109	109	
1867	−0·203	93	+0·080
1868	+0·245	131	
1869	+0 272	98	
1870	+0·122	126	
1871	+0·306	113	
1872	−0·037	120	
1873	+0·408	124	
1874	+0·762	106	+0·456
1875	+0·728	110	
1876	+0·675	112	
1877	+0·683	87	
1878	+0·816	85	
1879	+0·748	77	
1880	+0·861	115	+0·79
1881	+0·759	124	

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On the Change in the adopted Unit of Time. By Major-General J. F. Tennant, R.E., F.R.S.

Mr. Stone's papers on the cause of the large error in our modern Tables, and the criticisms I have seen on them, have led me to consider the subject; and I put my idea on paper because it may simplify the question to some. The result is the same as Mr. Stone's, and the reasoning, though independently thought out, is in much the same line as part of his second paper in the *Monthly Notices*.

For many years past the use of the Sun for the purpose of regulating clocks in an Observatory has ceased. Practically all observations are made in Sidereal Time, and the Mean Time where required has been deduced from it; and even where series of observations have been used in the investigation for Tables the Times have been reinvestigated, the clock-errors having been determined by the observations of stars. Therefore the Mean Times used have essentially depended on the length of the mean solar day in seconds of sidereal time. The Julian year taken at 365·25 mean solar days therefore depends on this time. The relation between the solar and sidereal days has been practically till recently that determined by Bessel, and it is from observations dated in what we may call Besselian Mean Time that the investigations of physical astronomy have deduced the data of modern Tables.

One of the discoveries from these investigations has been that the mean motion of the Sun and the length of the mean solar day in sidereal seconds dependent on it were not sufficiently accurate. The result of introducing the new Tables has been that, while the sidereal day and second are unchanged, the unit of Observatory Mean Time has become the Leverrian solar day* and year instead of the Besselian, and there has also been a small jump. Now, the data of, say, Leverrier's Tables having been determined from observations dated in Besselian Time should be consistent with fresh observations so dated. When compared with those dated by the Observatory clocks they do not agree, there is an apparent error of the Tables, which is really due to the fact that we are comparing a theoretical state of things at a Besselian date with the actual state at a Leverrian date nominally the same.

The Epoch of Leverrier's Elements is Jan. 1·0 1800 in Besselian Time, which is not the same physical instant as Jan. 1·0 1800 of Leverrian Time or that of the present *Nautical Almanac*, and the Elements require correction for the difference.

* The Mean Year or Mean Interval between two passages of the Sun through the first point of *Aries* is determined by observation as 366 Sidereal days $+n$ seconds, and this necessarily equals 365 Mean Solar days $+n$ Solar seconds. If n be wrongly determined, the change will not be in the length of the Sidereal day and second, but in that of the Solar day and second.

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The mean motions, again, are for Besselian mean days and Julian years, and to make them correct for the dates of our present *Nautical Almanac* they require to be multiplied by

$$\frac{\text{Sidereal Seconds in Leverrian Mean Day}}{\text{Sidereal Seconds in Besselian Mean Day}}$$

Hansen's or other Tables of the Moon will require a corresponding set of corrections to adapt them to our Observatory Times.

It is, however, unnecessary to correct the Tables. All we need do is to recollect that the data in our present *Nautical Almanac* are the direct result of the Tables, and that they must be corrected for the difference between the Mean Time used in making the Tables and the Leverrian Mean Time of the Observatories. Thus, if the difference between the Mean Time which Hansen's Tables are referred to and Leverrian Time is n^s on any day, the latter being the smaller we must compare the Moon's Right Ascension at Transit with that which according to the almanac she would have had n^s after the Transit.

When the data of the Tables have been entirely determined from observations reduced as I have described, then the Tables require only this correction of date. But there have been used some isolated old observations such as eclipses of the Sun in the Lunar Tables and Transits of *Venus* and *Mercury* whose times can only be determined by solar observations, or are given by observers in Apparent Time without means of referring to stars. In these cases the Mean Times are Leverrian, or *True* Mean Times, within the limits of observation, and we should expect a discordance with the calculations made from the provisional data. By using them to correct the data of the Tables on the hypothesis that these times were Besselian, error has been introduced. A marked case is that of the Secular Acceleration of the Moon's Mean Motion which has been determined practically from very old eclipses compared with the Tables whose mean motion came from modern observations.

1883, *Oct.* 31.

Remarks on Mr. Stone's Explanation of the Large and Increasing Errors of Hansen's Lunar Tables by means of a supposed Change in the Unit of Mean Solar Time. By Prof. J. C. Adams, M.A., F.R.S.

In some recent communications to the Royal Astronomical Society Mr. Stone contends that the mean solar day in use before 1864—when Le Verrier's Solar Tables were substituted for Bessel's in calculating the sidereal time at mean noon given in the *Nautical Almanac*—differs from the mean solar day adopted since that time.

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In the *Monthly Notices*, vol. xliii. p. 403, Mr. Stone states that the consequent error in our present reckoning in time is increasing at about the rate of $1^s.46$ per annum, and in the same volume, p. 335, he adduces this supposed error in explanation of the increasing errors of Hansen's Lunar Tables.

That this view of Mr. Stone's is erroneous may, I think, be shown by very simple considerations.

The only mean Sun known to astronomers is an imaginary body which moves uniformly in the equator at such a rate that the difference between its Right Ascension and that of the true Sun consists wholly of periodic quantities.

These periodic terms are due to the obliquity of the ecliptic, the eccentricity of the Earth's orbit, and also to the small perturbations of the Earth's motion about the Sun.

The difference between the Right Ascensions of the two bodies at any moment is called the Equation of Time.

The instant of Mean Noon is determined by the transit of this imaginary Mean Sun over the meridian of a given place just as the instant of Apparent Noon is determined by the transit of the true Sun over the same meridian.

Hence, the mean time, according to the definition of it above given, may be determined by observation of the transit of the true Sun over the meridian, subject only to the small error to which all transit observations are liable, and also to the extremely small error which is possible in the theoretical expression for the equation of time. When this mode of determining the mean time is employed, no accumulation of error in proportion to the interval of time from a given epoch is possible.

If, as it is frequently convenient to do, we wish to determine the mean solar time by means of the sidereal time supposed to be known, without having to make a transit observation of the Sun, we must employ the sidereal time at mean noon calculated from the proper formula or from the Solar Tables. This sidereal time at mean noon is equal to the Sun's mean longitude at mean noon corrected by the equation of the equinoxes in Right Ascension.

In order to find the mean time correctly in this way it is necessary to employ the correct value of the Sun's mean longitude, and any error in the assumed value of this quantity will produce an equivalent error in the mean time deduced.

Any such error can be at once checked and corrected by observation of the Sun's transit over the meridian.

If we wilfully refuse to check our results by solar observations, the error in the determination of the mean time by means of the sidereal time would, no doubt, increase in proportion to the interval of time from a certain epoch. Practically, however, it would be intolerable to use Solar Tables which were grossly erroneous, and long before the error of time became important the tables would be replaced by more accurate ones.

For many years previously to 1864 Bessel's formula had been

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employed in the *Nautical Almanac* for the calculation of the sidereal time at Greenwich mean noon.

In 1864 the error of Bessel's formula amounted to rather more than half a second of time, and accordingly in that and subsequent years the sidereal time at mean noon was deduced from Le Verrier's Solar Tables, which gave much more accurate results.

Now it is contended by Mr. Stone that by the change thus introduced into the *Nautical Almanac* the unit of mean solar time was practically altered to such a degree that at the end of 1881 the difference in the count of mean solar time amounted to nearly 27 seconds, and that the difference is increasing at the rate of about 1·46 seconds per annum.

It is clear, therefore, that if no such change had been made in the *Nautical Almanac*—that is, if Bessel's formula had continued to be employed—no such change of the unit of time would have taken place.

Let us see then, what difference this would have made in the count of mean solar time as derived from sidereal time when compared with the count found by means of our present *Nautical Almanac*.

Bessel's formula for the sidereal time at Greenwich mean noon of Jan. 1 in any year is given in the prefaces to the *Nautical Almanacs* from 1834 to 1863 inclusive. In 1864 and subsequent years the sidereal time at Greenwich mean noon is derived from Le Verrier's tables.

The following little table shows the sidereal time at Greenwich mean noon of Jan. 1 as calculated for every fifth year from 1860 to 1885 by Bessel's formula, and as taken from the several *Nautical Almanacs*:—

	By Bessel's Formula.	From *Nautical Almanac*.		Diff.
	h m s	h m s		s
1860	18 41 28·87	18 41 28·87	Bessel's formulæ employed	0·00
1865	18 44 35·36	18 44 35·92	Le Verrier's Tables employed	0·56
1870	18 43 43·87	18 43 44·44	,, ,,	0·57
1875	18 42 54·47	18 42 55·06	,, ,,	0·59
1880	18 42 5·95	18 42 6·56	,, ,,	0·61
1885	18 45 11·73	18 45 12·37	,, ,,	0·64

Hence we see that the difference of sidereal times at mean noon in consequence of the change from Bessel's formula to Le Verrier's Tables, which amounted to $0^s{\cdot}56$ in 1865, had increased to $0^s{\cdot}64$ in 1885. That is, the difference increases at the rate of $0^s{\cdot}08$ in twenty years, or of $0^s{\cdot}02$ in five years.

But according to Mr. Stone's theory as shown in his tabular comparisons of mean solar times computed from sidereal times by means of the *Nautical Almanac* and of those sidereal times

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"corrected to agree with Bessel's sidereal times," the differences would be as follows:—

	s		s
1865	2·0	1875	16·6
1870	9·3	1880	23·9

and at the end of 1881 the difference would have increased to $26^s{\cdot}8$; so that the increase in five years would be $7^s{\cdot}3$ instead of $0^s{\cdot}02$ as above. In fact the difference according to Mr. Stone's theory is just 365 times as great as it should be.

The origin of this enormous discrepancy between Mr. Stone's theory and the fact is readily seen by considering that mean solar time is measured, not by the Sun's mean motion in *longitude*, as Mr. Stone's theory supposes, but by the motion of the mean Sun in *hour angle*, which is about 365 times greater in amount. Hence any small error in the determination of the Sun's mean motion in longitude causes a proportionate error of only about a 365th part of the amount in the interval of mean solar time as inferred from the interval of sidereal time. In fact, if n denote the Sun's mean motion in longitude in a mean solar day, then the length of the mean solar day will be to the sidereal day in the ratio of

$$360° + n : 360°.$$

If now $n + dn$ denote another slightly different determination of the Sun's mean motion in longitude in a mean solar day, the ratio of the length of a mean solar to that of a sidereal day will become

$$360° + n + dn : 360°.$$

Hence the measure of a mean solar day when expressed in sidereal time will be increased in the ratio of

$$360° + n + dn : 360° + n,$$

or

$$1 + \frac{dn}{360° + n} : 1.$$

Since 360° is nearly 365 times n, this ratio will be

$$1 + \frac{1}{366}\ \frac{dn}{n} : 1 \text{ nearly},$$

Whereas, according to Mr. Stone's theory, this ratio should be

$$1 + \frac{dn}{n} : 1.$$

It has been already remarked that it is convenient practically to determine the mean solar time from the sidereal time, but in order to do this correctly, it is of course necessary

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to employ the correct value of the Sun's mean longitude. At the present time Bessel's value of the Sun's mean longitude is about $0^s.6$ in error, and therefore the mean solar time inferred by means of it from the sidereal time would be in error to the same amount. The mean longitude found from Le Verrier's Tables is much nearer to the truth, and therefore the mean solar time found from the sidereal time by using this value would be much more nearly correct.

It must not be forgotten however that, as we have already stated, the mean solar time may be derived from observations of the transit of the Sun over the meridian, without employing the sidereal time at all. Apparent solar time, which is found directly from observation of the Sun is converted into *mean* solar time by applying the equation of time, which is known from the solar theory, without reference to the sidereal time.

On the Change in the Unit of Time implied in the Substitution of Hansen's for Bessel's Expression for the Longitude of the mean Sun. By Prof. A. Cayley, M.A., F.R.S.

I have before me Mr. Stone's paper "An Explanation of the Principal Cause of the Large Errors &c.", *Monthly Notices*, vol. xliii. (1883) pp. 339–345, in which Mr. Stone arrives at the conclusion that the effect of the substitution of Hansen's expression for the longitude of the mean Sun in place of that of Bessel is to change the unit of time in the ratio of $1+x$ to 1, where

$$x = -\frac{0''.055871}{1296027''.674055}.$$

Prof. Adams considers, I think rightly, that the true value of x is about $\frac{1}{365}$ of this value.

I wish in the present paper to present the question in what appears to me to be the most distinct manner.

Bessel's expression for the longitude of the mean Sun, 1850 Jan. 1, Paris mean noon $+t$ is

$$l = 280^\circ\ 46'\ 36''.12 + 1296027''.618184\,.\,t + 0''.0001221805\,.\,t^2,$$

say $\quad = \quad$ " $\quad + Lt \quad$ "

the unit of time being *supposed* to be a Julian year of 365·25 mean solar days, and only the term in t being here attended to.

I infer that according to Bessel the Sun's mean hour-angle from the meridian of Paris would be

$$h, = 365{\cdot}25 \times 360^\circ\,.\,t,$$

viz. t increasing by $\frac{1}{365{\cdot}25}$ (=one mean solar day) h would increase by 360°. Say this value is $h = Ht$.

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The Right Ascension of the mean Sun is equal to the mean longitude l; and hence the Right Ascension of the meridian of Paris is $h+l, =(H+L)t$; or reckoning from a fixed equinox instead of the mean equinox, this is $h+l$—a term depending on the precession $=(H+L-\varpi)t$; $\varpi = 50''{\cdot}21129(1-\cos\omega)$, where $50''{\cdot}21129$ is Bessel's value for the general precession.

Hence

$(H+L-\varpi)t$ = Bessel's value for Earth's rotation in t Julian years.

Similarly, Hansen's expression for the longitude of the mean Sun, 1850 Jan. 1 Paris mean noon $+t'$, is

$$l' = 280°\ 46'\ 43''{\cdot}20 + 1296027''{\cdot}674055\,.\,t' + 0''{\cdot}0001106850\,.\,t'^2$$

say $\quad = \quad$ " $\quad + L't' \quad$ "

the unit of time being *supposed* to be a Julian year of $365{\cdot}25$ mean solar days; and only the term in t' being here attended to.

I infer that according to Hansen the Sun's mean hour-angle from the meridian of Paris would be

$$h' = 365{\cdot}25 \times 360°\,.\,t',$$

viz. t' increasing by $\frac{1}{365{\cdot}25}$ (=one mean solar day) h' should increase by 360°. Say we have $h' = H't'$. H' is=H, but for convenience I use the accented letter.

As before, the Right Ascension of the meridian of Paris, reckoned from a fixed equinox, is $h'+l'$—a term depending on the precession, $=(H'+L'-\varpi')t'$ $\varpi' = 50''{\cdot}224(1-\cos\omega)$, where $50''{\cdot}224$ is Hansen's value of the general precession.

Hence

$(H'+L'-\varpi')t'$ = Hansen's value of Earth's rotation in t' Julian years.

Now, Bessel's mean Sun is not Hansen's mean Sun, and a Julian year (Bessel) is not=a Julian year (Hansen). In order to determine the ratio of the two Julian years (or say the ratio of the two units of time) Mr. Stone compares the motions in longitude Lt, and $L't'$ of the two mean Suns; but this assumes that the two mean Suns are identical, *which, as just remarked, is not the case.* Admitting that they were so, and neglecting with Mr. Stone the difference of precession the ratio would be

$$\frac{L'}{L}, \quad = 1 + \frac{L'-L}{L}, \quad = 1 + \frac{0''{\cdot}055871}{L},$$

The proper equation for the comparison of the units is afforded by the expressions for the time of the Earth's rotation; consider a *definite* interval of time (=about one Julian year), and say this is the adopted Julian year; and suppose

Julian year (Bessel) $= (1+x)$ adopted year.

" (Hansen) $= (1+x')$ "

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Then in one adopted year

$$\text{Rotation (Bessel)} = \frac{H + L - \varpi}{1 + x}$$

$$\text{,, (Hansen)} = \frac{H' + L' - \varpi'}{1 + x'}.$$

And we thus have

$$\frac{1 + x'}{1 + x} = \frac{H' + L' - \varpi'}{H + L - \varpi}, \quad = 1 + \frac{H' - H + L' - L - (\varpi' - \varpi)}{H + L - \varpi}.$$

Here

$$H' = H, \ = 365{\cdot}25 \times 360^\circ = 131490^\circ, \ = 473364000'', \ H + L = 474660027,$$
$$= \text{about } 366L:$$

hence we have

$$\frac{1 + x'}{1 + x} = 1 + \frac{L' - L - (\varpi' - \varpi)}{366L}.$$

Or neglecting as before $\varpi' - \varpi$, the ratio is

$$= 1 + \frac{0''{\cdot}055871}{366L};$$

viz. the alteration of ratio is $= \frac{1}{366}$ of the value found by Mr. Stone. As regards the term $\varpi' - \varpi$, Bessel's precession is $50''{\cdot}21129$, Hansen's $50''{\cdot}224$, whence

$$\varpi' - \varpi = 0''{\cdot}01271\ (1 - \cos 23^\circ\ 28') = 0''{\cdot}01271 \times {\cdot}0898, \ = 0''{\cdot}00114;$$

so that instead of $0''{\cdot}055871$ the numerator should be $0''{\cdot}05473$.

The Orbit of the Great Comet (b) 1882. By J. Morrison, M.A., M.D., Assistant on the *American Ephemeris and Nautical Almanac*, Washington.

(*Communicated by the Foreign Secretary.*)

Ever since the discovery of this remarkable comet in the early part of September 1882, numerous efforts to determine the elements of its orbit have been made by astronomers in different parts of the world, and in no similar case has there been, we believe, a greater disparity in the results obtained. An orbit like the one under consideration, which is so situated with respect to the orbits of the planets that the effect of planetary perturbations is absolutely inappreciable, must furnish a good example of pure and undisturbed elliptic motion, and therefore we would naturally expect that a system of elements computed from three or four good observations suitably selected would very nearly, if not exactly, represent its motion. In this expectation, however, we are disappointed, for no orbit has hitherto been published which completely satisfies the observations, especially those made after the middle of October or thereabouts. From the time of discovery until about this date the nucleus

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presented a tolerably well-defined circular disk which could be as easily observed as that of a planetoid; but soon afterwards extraordinary changes began to take place in the structure of the nucleus, which, from being circular in outline, became oval or elliptical, with from two to four or five bright points or centres of condensation. Thus, from about October 20 until May 26 the length of the nucleus varied from 57″ to about 135″, and the position-angle of its major axis varied from 113° to 255° —which is nearly at right angles to the meridian—hence it is not difficult to see that an error of 30″, or even more, in R.A., may exist in any observation made about this time, according to the point of the nucleus observed. The error in Declination will be much less than that in R.A. for the reason given above. The following two observations made at the Litchfield Observatory, Clinton, N.Y., and at the Naval Observatory, Washington, are here adduced to show the inconsistency of the results obtained when different points of the nucleus were observed :—

Washington Mean Time.	R.A.	Dec.
	h m s	° ′ ″
1882, Oct. 24·712191	10 6 25·78	−17 6 48·2
24·739461	10 5 52·29	−17 5 6·8

According to the geocentric motion of the comet at that time these observations are inconsistent with each other, and therefore of little or no value in the present investigation. That the discrepancy is due to this cause alone is evident from the fact there were no errors in the position of the comparison star or in the reduction of the observations, which were re-examined by the observers. It is therefore not at all suprising that periods differing very widely from each other have been obtained by different computers, according to the observations employed. Thus, from post-perihelion observations Mr. Chandler obtained periods of 3115 days and 4070 years respectively, and between these extremes we have periods of 269, 652, 712, 793, 843, 997, and 1376 years. The most probable period is between 712 and 793 years, as will appear presently. With the view of comparing the elements obtained from observations made after perihelion passage with those deduced from observations made before and after that epoch, we select the following observations:

	Washington Mean Time.	R.A.	Dec.	Where made.
	d	c ′ ″	° ′ ″	
1	1882, Sept. 8·113405	144 16 52″5	− 0 57 46″4	Windsor
2	8·452672	144 59 51·4	− 0 56 30·0	The Cape
3*	14·348690	161 28 20·0	+ 0 4 12 9	Melbourne
4*	16·368673	170 39 26·3	+ 1 12 4·8	,,
5	16·936700	Ingress on Sun's disk.		The Cape
6*	19·969786	168 34 57·6	− 0 34 28·5	Washington
7	Oct. 8·720457	157 1 37·9	−10 40 21·9	,,

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	Washington Mean Time.	R.A.	Dec.	Where made.
	d	h m s	° ′ ″	
8	27·710103	150 31 33·9	−18 14 31·1	Clinton
9*	Nov. 15·740418	141 57 40·8	−24 49 18·9	Washington
10*	24·700923	136 34 3·3	−27 21 26·7	,,
11*	Dec. 11·619921	124 5 25·3	−30 16 28·1	,,
12	1883, Jan. 13·778694	100 3 54·9	−26 48 39·5	Windsor
13	Feb. 28·304652	87 56 18·0	−15 16 7·1	Washington
14	Apr. 6·097128	89 31 28·9	− 9 4 49·2	Palermo
15	May 6·252250	93 32 12·6	− 6 21 30·3	Cordoba
16	26·257880	97 11 12·6	− 5 30 0·0	,,

These observations embrace nearly the entire period of visibility of the comet. No. 7 was made with the equatorial, the comparison star being a well-known one, whose position was subsequently verified by the Transit Circle; No. 8 is marked by the observer as "very good;" in Nos. 9 and 11 the "main point of condensation near the following end of the nucleus was observed," and in 13 "the middle point between the second and third points of the nucleus." No remarks respecting the remaining observations are made by the respective observers.

From Nos. 6, 7, and 13 we have obtained the following system of elements:—

I.

T	1882, Sept. 17·00858 Washington M. T.
ω	69 37′ 27″·7 Mean Equinox of 1882·0.
Ω	346 1 38·8
i	38 0 7·7
log. q	7·8922566
log. e	9·9999660
log. a	1·9992346
P	997·36 years. Motion retrograde.

These elements, although computed with great care, do not represent the motion in the neighbourhood of perihelion. The residuals for the middle place are much greater than what would likely be due to the unavoidable errors of computation and observation. Among quite a number of places computed the smallest residuals obtained were the following:—

		$\Delta\lambda \cos\beta$	$\Delta\beta$
		″	″
3	Sept. 14·34+	+27·21	+17·20
9	Nov. 15·74+	−24·49	+12·53

It is very probable that a slight error in the third place has to some extent vitiated these results. The interval between the first and third observations is 161·3247 days, during which time the comet described an arc of only 16° 12′·1, an amount of motion far too small to permit of an accurate determination of

* Meridian observations.

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the elements, especially in orbits like the present one, whose eccentricity is very large.

For the computation of a second system of elements, we will take observations which give a large amount of motion, and therefore select the three meridian observations of September 14, September 19, and November 15; the last being the first meridian observation made after the comet had ceased to be visible in daylight, and had passed from the immediate vicinity of the Sun. At this time, too, the process of disintegration in the nucleus had not advanced very far, and from the remark appended to the observation it is very probable that it is very nearly as accurate as either of the other two about which there is no doubt.

In order to show the degree of accuracy to which the computation has been carried we here give the results of the last two hypotheses. The notation is the same as that used by Gauss in his *Theoria Motus*, with the exception that we have employed u to denote the argument of latitude and ρ the curtate distance. The ratio of the sector to the triangle corresponding to the first and second observations, and also to the first and third, is *negative* and numerically less than unity.

	I.	II.
log. P	9·5492503*n*	9·5492433*n*
log. Q	8·4458408*n*	8·4458447*n*
$\omega+\sigma$	2° 25′ 49″·788	2° 25′ 50″·016
log. Qc sin ω	0·3691234	0·3691230
ε	48° 1′ 1″·943	48° 1′ 1″·936
log. r'	9·3402558	9·3402558
log. ρ'	0·0539758	0·0539758
log. ρ	9·9972618	9·9972631
log. ρ''	0·0734265	0·0734271
log. r	9·3097846	9·3097852
log. r''	0·2226261	0·2226265
Ω	346° 0′ 41″·385	346° 0′ 41″·254
i	38 0 7·03	38 0 7·19
$\frac{1}{2}(u''-u)$	164 52 45·703	164 52 47·13
$\frac{1}{2}(u'-u)$	157 55 32·977	157 55 34·27
$\frac{1}{2}(u''-u')$	6 57 12·726	6 57 12·86
log. η	0·1412227	0·1412226
log. η''	9·5876330*n*	9·5876319*n*
log. P$_1$	9·5492420*n*	9·5492433*n*
log. Q$_1$	8·4458464*n*	8·4458450*n*
x	+0·0000083	0·0000000
y	−0·0000056	−0·0000003

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The three values of log p, the semi-parameter, are each equal to 8·1905156, which proves the accuracy of the entire computation up to this point. On completing the computation of the elements from these results, we have the following system:—

II.

T	1882, Sept. 17·010163 Washington M.T.
ω	69° 35′ 30″·483 Mean Equinox of 1882·0.
Ω	346 0 41·254
i	38 0 7·19
log. q	7·8895067
log. e	9·9999578
log. a	1·9016841
P	712·10 years. Motion retrograde.

The residuals for the middle place are zero. It now remains to be seen how closely these elements satisfy the above observations.

The residuals are given in the following table, where C—O denotes, as usual, the difference between the *computed* and the *observed* places, and Δ the distance from the earth.

C—O.

	$d\lambda \cos\beta$	$d\beta$	log. Δ
	″	″	
1	− 3·71	− 0·28	0·0604274
2	− 0·75	+ 1·02	0·0569198
4	+ 1·60	+ 0·82	9·9900265
7	− 0·16	− 0·44	0·1406414
8	+ 1·01	+ 0·37	0·1662759
10	+ 0·92	− 1·12	0·1766315
11	+ 13·41	+ 6·45	0·1916736
12	−61·20	+37·52	0·2798283
13	−45·68	+ 3·82	0·4629200
14	−52·12	−27·24	0·5880931
15	−64·07	−31·20	0·6640691
16	−54·12	−20·74	0·7022010

Transit over the Sun's Disk.

The ingress on the Sun's disk took place on September $17^d\ 4^h\ 50^m\ 58^s$, Cape mean time, according to Mr. Finlay, and at $4^h\ 50^m\ 52^s$, according to Dr. Elkin (*Monthly Notices*, November 1882). Adopting the former and applying the correction for aberration, which is $-0^d·005698$, we have for the position of the comet the following results:—

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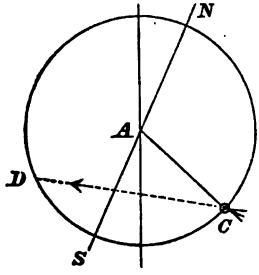
Washington Mean Time.	λ	β	log. Δ
1882, Sept. 16·931001	174° 27′ 19″·83	−0° 13′ 12″·39	9·9952130
Sun's position, ⊙ ...	174 36 19·06	−0 0 0·22	
Difference ...	0 8 59·23	13 12·17	

Distance between the centres	15′ 58″·28
Sun's semi-diameter	15 57·73
Comet's distance from Sun's limb ...	0·55

The diagram shows the position of the comet with respect to the Sun at the time of ingress.



N S is a meridian; C D the apparent path of the comet; N A C the angle of position, 122° 23′·7 from the north point of the Sun's limb towards the west.

Since this second system of elements represents the motion of the comet with great accuracy from the time of discovery till about the middle of November, and from this date until the last observation here recorded, with all the precision that can be expected under the circumstances, we may also give the following facts as tending to show more strongly perhaps the characteristics of this remarkable body.

At the time of ingress on the Sun's disk the comet's true anomaly was 92° 7′ 29″·668, which was described in *one hour and fifty-four minutes*. From the time of the first observation made at Windsor by Mr. Tebbutt until May 26th, an interval of 260·1447 days, the comet described 340° 22′·7 of its orbit, but it will require more than seven centuries to describe the remaining 19° 37′·3. Taking the mean solar parallax at 8″·848, the value adopted in the English and American *Nautical Almanacs*, the comet's perihelion distance was 716200 miles from the Sun's centre, or 285500 miles from the surface.

The velocity at perihelion was 295·36 miles per second, which is only ·0160 miles per second less than that due to a parabola at the same perihelion distance.

When the comet reaches aphelion, it will have plunged down into space a distance equal to 160 radii of the Earth's orbit, or 5½ times the radius of *Neptune's* orbit, at which enormous distance its velocity will be only 75 feet per second.

At the time of Dr. Elkin's first observation on Sept. 8, the apparent diameter of the coma was estimated at from 40″ to 50″, and the strongly condensed cone or nucleus in the centre at from 10″ to 15″. Assuming the means of these measurements,

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we easily find the diameter of the coma at that time to have been 22500 miles, and of the nucleus 7600 miles, or a little less than that of the Earth. As the comet approached the Sun, the nucleus contracted, and about half an hour before its disappearance on the solar disk Mr. Finlay found by two micrometric measures the apparent diameter to be 4″, while Dr. Elkin estimated it at some 5″. The former of these gives a diameter of 1770 miles, and the latter 2215 miles, or a little greater than that of the Moon.

The second system of elements here presented proves conclusively that an orbit can be found which satisfies the motion of the body equally well both before and after perihelion; and therefore we may safely conclude that no retardation occurred during the perihelion passage; or, if there was, it is absolutely inappreciable. There are two other systems of elements which differ considerably in the periodic time from our second system, but the agreement of some of the other elements is very remarkable. We refer to those of Prof. Frisby and Dr. Kreutz; the former founded his calculations on the observations of Sept. 19, Oct. 8, and Nov. 24, and the latter on normal places derived from observations extending from Sept. 8 to Nov. 15. The three systems just referred to are here given for comparison.

	Dr. Morrison.	Prof. Frisby.	Dr. Kreutz.
T*	1882, Sept. 17·010163	1882, Sept. 17·0088	1882, Sept. 17·00993
ω†	69° 35′ 30″·48	69° 36′ 12″·79	69° 36′ 1″·50
Ω	346 0 41·25	346 1 7·91	346 1 27·20
i	38 0 7·19	38 0 7·84	38 0 19·9
log. q	7·8895067	7·8904739	7·8894760
log. e	9·9999578	9·9999606	9·9999610
log. a	1·9016841	1·9331366	1·9505596
P	712·10 yrs.	793·869 yrs.	843·10 yrs.
	Mean Equinox 1882·0	Mean Equinox 1882·0	Mean Equinox 1882·0

* Washington Mean Time. † Motion retrograde.

This comet is probably identical with the comet of 370 B.C., as already pointed out by Mr. Maxwell Hall. If we divide the interval between 370 B.C. and 1882 A.D. by three we get a period of 751 years, which is very nearly the arithmetic mean between our period and that of Prof. Frisby. This period would give a return in A.D. 1131 or 1132, in both of which years a great comet appeared (*Chinese Annals*).

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Observations of Pons's Comet made at the U.S. Naval Observatory, Washington, with the 9-inch Equatorial.

By Commander W. T. Sampson and Prof. E. Frisby.

(*Communicated by the Secretaries.*)

1883	Washington M.T.	Comet−✳ Δα	Comet−✳ Δδ	No. of Comp.	Comet α app.	log. (p×Δ)	Comet δ app.	log. (p×Δ)	Obs.	Comp. Star.
	h m s	m s	′ ″		h m s		° ′ ″			
Sept. 23	8 8 24·5	+0 1·19	− 2 10·3	15·3	16 25 20·93	9·89564	+60 31 56·8	9·72026*n*	S	1
24	11 19 47·3	+4 57·92	− 2 12·9	15·3	16 25 28·49	9·94552	60 16 47·8	0·76095	S	2
26	7 42 33·3	− 31·98	−10 10·6	25·5	16 25 47·71	9·87342	59 52 22·8	9·82982*n*	S	3
26	9 56 0·3	+0 16·86	+ 7 7·4	18·4	16 25 47·07	9·96138	59 51 16·0	0·41280	F*	4
28	8 9 12·4	−2 4·47	− 9 35·9	20·3	16 26 17·22	9·91121	59 26 16·6	9·38115	F	5
30	9 39 24·7	+3 32·05	− 5 24·4	5·1	16 27 5·26	9·94484	58 58 54·6	0·57845	S	6
30	9 50 37·1	+3 32·02	− 5 12·4	5·1	16 27 5·23		58 58 42·6		S	6
Oct. 2	9 30 9·2	+1 39·78	+ 3 46·8	25·5	16 28 2·02	9·30293	58 33 52·3	9·51845*n*	S	7
2	9 30 9·2	+3 12·29	+ 3 19·9	25·5	16 28 2·41		58 33 51·8		S	7
8	7 43 21·6	−0 48·60	+ 6 4·3	25·5	16 32 2·33	9·88741	57 16 55·5	9·73798	S	9
9	7 17 34·7	0 0·00	− 6 27·7	10·2	16 32 50·90	9·87016	57 4 23·3	9·39904	S	9
9	7 24 12·3	0 0·00	− 6 31·6	10·2	16 32 50·90		57 4 19·4		S	9
10	7 22 41·5	−9 20·48	− 7 57·0	15·3	16 33 43·74	9·86011	56 51 49·0	7·65475	S	10
11	7 54 46·7	+8 5·37	−12 44·3	15·3	16 34 39·90	9·89986	56 38 56·5	0·09116	S	11
11	8 6 16·5	+8 6·21	−12 50·3	10·2	16 34 40·74		56 38 50·5		S	11
19	9 3 14·1	−6 37·23	+ 4 45·5	10·2	16 43 45·76	9·90472	55 1 19·7	0·48745	F	12
24	6 52 24·8	+2 7·06	+ 5 20·8	15·3	16 50 49·66	9·84559	54 4 23·8	9·87235	S	13

* Made with 26-in. Equatorial.

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1883.	Washington M.T.	Comet − ✱ Δα	Comet − ✱ Δδ	No. of Comp.	Comet α app.	log. ($p \times \Delta$)	Comet δ app.	log. ($p \times \Delta$)	Obs.	Comp. Star.
	h m s	m s	′ ″		h m s		° ′ ″			
Oct. 24	7 9 40·5	+1 58·15	− 9 57·3	15·3	16 50 52·28	9·85554	+54 3 58·0	9·99734	S	14
24	7 43 13·8	+2 8·19	− 6 51·0	15·3	16 50 55·44	9·88111	54 3 43·4	0·28026	S	15
26	7 9 43·0	−4 36·67	−10 50·6	25·5	16 54 4·86	9·85996	53 41 9·2	9·51435	S	16
27	7 24 50·3	−5 30·99	+ 6 1·3	20·4	16 55 47·79	9·73856	53 29 48·1	0·25273	S	17
29	9 10 27·2	+2 2·91	− 6 46·3	25·5	16 59 27·40	9·87621	53 6 25·0	0·63868	S	18
30	6 45 21·5	−3 36·47	+ 6 10·5	20·4	17 1 5·93		52 56 31·7		F	19
30	6 45 21·5	−5 5·84	+ 9 20·4	11·3	17 1 5·68		52 56 29·7		F	20
30	6 45 21·5	−5 28·43	+ 3 15·3	10·4	17 1 6·65	9·83976	52 56 31·1	0·00232	F	21
30	7 27 59·8	+2 38·29	+ 9 33·3	15·3	17 1 10·82		52 56 12·9		S	22
31	7 20 16·9	−1 38·18	− 5 15·5	15·2	17 3 4·19		52 45 5·6		F	19
31	7 20 16·9	−3 7·77	− 2 58·9	14·2	17 3 3·73		52 45 10·3		F	20
31	7 20 16·9	−3 31·13	− 8 10·2	9·2	17 3 4·11	9·86293	52 45 5·5	0·27512	F	21
31	7 49 19·2	+4 32·39	− 1 50·6	10·2	17 3 4·89		52 44 48·8		S	22
Nov. 1	8 13 14·8	+2 24·60	+ 2 45·2	20·4	17 5 4·43	9·87821	52 33 42·4	0·50105	S	23
1	8 13 14·8	−0 11·65	+ 5 17·2	20·4	17 5 4·80	9·87821	52 33 42·2	0·50108	S	24
3	8 23 28·3	−1 46·64	−14 59·7	20·4	17 9 10·03	9·87485	52 11 33·5	0·54866	S	25
4	7 11 11·0	−2 1·52	+ 5 2·2	20·4	17 11 11·40	9·85598	52 1 26·8	0·27695	S	26
5	7 12 20·8	−4 20·30	+11 38·7	15·3	17 13 22·43	9·85560	51 50 33·2	0·29402	F	27
9	7 18 43·8	−4 8·48	− 4 39·7	10·2	17 22 42·53		51 6 47·0		S	28
9	7 18 43·8	−3 25·84	− 8 53·8	10·2	17 22 42·96	9·85479	51 6 51·2	0·36871	S	29

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1883.	Washington M.T. h m s	Comet—* Δα m s	Δδ ′ ″	No. of Comp.	Comet α app. h m s	log. (p × Δ)	Comet δ app. ° ′ ″	log. (p × Δ)	Obs.	Comp. Star.
Nov. 10	7 27 47·3	−0 55·61	− 2 7·7	15·3	17 25 13·17	9·85690	+50 55 49·4	0·41411	S	29
12	7 10 48·8	+1 1·15	+ 5 33·5	25·5	17 30 21·15	9·85098	50 34 15·1	0·39056	F	3
12	7 21 47·4	+0 1·88	− 2 42·0	15·3	17 30 22·67	9·85208	50 33 58·0	0·40286	F	31
12	7 48 38·8	+3 41·21	− 6 15·3	15·3	17 30 26·64	9·85862	50 33 48·3	0·50127	S	32
14	5 51 28·4	−2 25·86	+ 9 0·5	20·4	17 35 41·97	9·78291	50 12 41·5	9·84613	S	33
14	7 7 34·3	−6 11·46	+ 6 22·0	15·3	17 35 50·59	9·84441	50 12 8·6	0·36478	S	34
14	7 7 34·3	−4 58·75	+ 5 35·9	15·3	17 35 51·56	9·84439	50 12 7·2	0·36473	S	35
15	5 49 33·9	−3 31·09	− 4 25·0	15·3	17 38 30·96	9·78037	50 1 21·4	9·84912	S	34
15	5 49 33·9	−2 18·38	− 5 7·4	15·3	17 38 31·91	9·78035	50 1 23·4	9·84888	S	35
15	7 23 55·6	+0 33·66	− 3 1·3	20·4	17 38 41·44	9·84870	50 0 39·5	0·43837	F	33
15	7 41 19·5	−1 49·28	+ 4 27·4	15·3	17 38 44·67	9·85316	50 0 35·2	0·51666	F	36
16	6 26 11·8	+0 54·84	− 6 21·1	15·3	17 41 28·77	9·81964	49 49 46·5	0·19851	F	36
16	6 36 23·5	−2 2·92	+ 2 25·3	15·3	17 41 31·80	9·82423	49 49 49·5	0·23341	S	37
17	6 33 45·4	+0 56·44	− 9 11·0	15·3	17 44 31·15	9·82128	49 38 12·9	0·22918	S	37
18	7 32 18·0	−1 11·16	+ 7 51·0	20·4	17 47 43·11	9·84687	49 26 16·6	0·48177	S	38
19	5 51 54·6	+1 45·09	− 3 13·3	20·4	17 50 39·35	9·78054	49 15 11·8	9·95965	S	38
19	6 9 53·1	−2 56·69	+ 4 50·4	20·4	17 50 41·19	9·79928	49 15 2·6	0·10411	S	39
27	5 46 59·1	+0 3·95	− 0 25·5	25·5	18 19 6·02	9·76351	47 30 24·8	0·03048	S	40
27	6 1 7·5	+0 5·98	− 0 28·5	20·4	18 19 7·93	9·77887	47 30 21·9	0·12947	F	40
27	7 13 17·8	+0 18·39	− 1 22·4	20·4	18 19 20·45	9·82614	47 29 27·8	0·44808	S	40
28	5 44 1·7	−0 43·54	− 2 31·1	25·5	18 23 6·27	9·75792	47 15 38·8	0·01931	S	41
30	7 34 31·4	−2 59·98	− 0 4·6	25·5	18 31 45·94	9·82085	46 42 40·7	0·52792	F	42

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Assumed Mean Places of Comparison Stars.

Star.	α 1883·0.	δ 1883·0.	Authority.
	h m s	° ′ ″	
1	16 23 13·67	+60 34 2·4	Bonn
2	16 20 31·19	60 18 43·4	Lal. Fed.
3	16 26 20·28	60 2 15·8	$\frac{\text{O. Arg. N.} + \text{Bonn}}{2}$
4	16 25 30·79	59 43 51·1	Bonn
5	16 28 22·29	59 35 35·0	O. Arg. N.
6	16 23 33·87	59 3 38·4	,,
7	16 26 22·88	58 29 48·8	,,
8	16 24 50·77	58 30 15·4	,,
9	16 32 51·57	57 10 34·8	,,
10	16 43 4·80	56 59 28·5	Berliner Jahrbuch
11	16 26 35·40	56 51 39·4	O. Arg. N.
12	16 50 23·58	54 56 17·7	,,
13	16 48 43·22	53 58 48·0	,,
14	16 48 54·77	54 13 40·2	,,
15	16 48 47·89	54 10 17·4	,,
16	16 58 42·12	53 51 43·9	,,
17	17 1 19·40	53 23 31·4	,,
18	16 57 25·10	53 12 56·3	Bonn
19	17 4 42·93	52 50 5·4	$\frac{\text{Bonn} + \text{Rümker}}{2}$
20	17 6 12·05	52 46 53·3	Bonn
21	17 6 35·62	52 52 59·7	,,
22	16 58 33·10	52 46 24·6	O. Arg. N.
23	17 2 40·37	52 30 42·1	Bonn
24	17 5 16·99	52 28 9·5	O. Arg. N.
25	17 10 57·22	52 26 17·3	,,
26	17 13 13·41	51 56 8·7	Lalande
27	17 17 43·18	51 38 38·2	Bonn
28	17 26 51·44	51 11 10·3	O. Arg. N.
29	17 26 9·21	50 57 41·0	Struve
30	17 29 20·40	50 28 25·5	Bonn
31	17 30 21·19	50 36 23·8	,,
32	17 26 45·86	50 39 47·9	O. Arg. N.
33*	17 38 8·16	50 3 24·2	,,

* Argelander's Declination for this star has been increased by 1 micrometer rev. = 47″·2.

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Star.	α 1883·0. h m s	δ 1883·0. ° ′ ″	Authority.
34	17 42 2·36	+50 5 29·1	O. Arg. N.
35	17 40 50·62	50 6 13·7	,,
36	17 40 34·25	49 55 50·8	,,
37	17 43 35·02	49 47 6·9	,,
38	17 48 54·51	49 18 8·0	,,
39	17 53 38·09	49 9 54·4	Bonn
40	18 19 2·06	47 30 31·0	O. Arg. N.
41	18 23 49·77	47 17 50·2	,,
42	18 34 45·80	46 42 24·6	,,

Comparison of Observations with Ephemeris.

Date.	Obs.−Comp. Δα s	Δδ ″	Date.	Obs.−Comp. Δα s	Δδ ″	Date.	Obs.−Comp. Δα s	Δδ ″
Nov. 1	−0·06	+20·0	Nov. 12	+1·60	+13·6	Nov. 17	+2·62	+14·7
,, 1	+0·26	+19·8	,, 14	+2·76	+16·7	,, 18	+2·82	+25·9
,, 3	+0·26	+11·4	,, 14	+1·14	+16·1	,, 19	+2·58	+18·7
,, 4	+0·73	+26·9	,, 14	+2·75	+24·5	,, 19	+2·16	+17·9
,, 5	+0·94	+27·5	,, 15	+1·90	+13·4	,, 27	+4·11	+ 9·4
,, 9	+0·96	+14·8	,, 15	+3·72	+28·8	,, 27	+3·72	+15·3
,, 9	+1·48	+17·6	,, 15	+3·15	+15·4	,, 27	+4·57	+ 6·6
,, 10	+1·20	+12·6	,, 15	+2·43	+20·7	,, 28	+4·03	+15·6
,, 12	+0·32	+24·1	,, 16	+0·61	+16·4	,, 30	+5·57	+ 9·3
,, 12	+0·60	+12·0	,, 16	+2·39	+23·6			

Unfavourable weather prevented the comet being seen before Sept. 23. On that evening I was surprised at its brightness, as it had been described as a faint object. The light was a bright yellow, without showing any decided nucleus. On the 24th the appearance was much the same as on the previous evening. On the 25th it was not seen. On the 26th its appearance had changed in a most marked manner. It was much fainter, and the light had lost its yellow colour.

Observations of Comet b, 1883 (*Pons-Brooks*), *made at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The observations were made with the East or Sheepshanks Equatorial (aperture 6·7 inches) by taking transits over two cross-wires at right angles to each other, and each inclined 45° to the parallel of declination.

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Gr. Mean Solar Time. 1883, Nov.	δ − ✱ R.A.	Corr. for Par. and Refrac. in R.A.	δ − ✱ N.P.D.	Corr. for Par. and Refrac. in N.P.D.	No. of Comp.	App. R.A.	App. N.P.D.	
			Observer: AD.					
d h m	m s	s	′ ″	″		h m s	° ′ ″	
10 6 52	+3 4·61	+0·36	−11 2·0	−2·2	2	17 24 37·61	39 0 48·8	*a*
7 1	−1 31·12	+0·38	− 0 21·0	−2·1	2	17 24 38·01	39 1 3·4	*b*
			Observer: T.					
17 7 7	+0 52·69	+0·39	− 6 30·4	−2·5	5	17 43 55·43	40 19 54·6	*c*
7 5	−0 22·86	+0·39	+ 7 1·8	−2·4	2			*d*
			Observer: H.					
19 6 58	+1 13·66	+0·41	+ 1 32·7	−2·6	1	17 50 7·95	40 43 6·2	*e*
9 10	+1 29·65	+0·40	+ 2 15·9	−4·4	6	17 50 23·94	40 43 49·4	*e*
9 16	−3 14·86	+0·39	− 5 52·4	−4·2	3	17 50 23·06	40 43 53·8	
			Observer. T.					
27 5 56	+0 16·30	+0·40	− 2 47·3	−2·3	5	18 18 17·38	42 25 39·9	*g*
6 8	−0 42·35	+0·43	− 2 39·9	−2·5	9	18 18 18·16	42 26 38·7	*h*

Assumed Mean Places of the Comparison Stars.

Star.	Star's Name.	R.A. 1883·0.	N.P.D. 1883·0.	Authority.
		h m s	° ′ ″	
a	Lalande 31840	17 21 33·44	39 12 6·2	Lalande
b	Arg. Z. + 50° − 2412	17 26 9·55	39 1 40·6	Bonn. Obs., vol. v.
c	Arg. Z. + 49° − 2689	17 43 3·02	40 26 41·7	,,
d	Anonymous			
e	Oeltz. Arg. (N) 17595	17 48 54·55	40 41 50·6	Oeltz. Arg. (N) 1842
f	Arg. Z. + 49° − 2714	17 53 38·13	40 50 4·0	Bonn Obs., vol. v.
g	Arg. Z. + 47° − 2621	18 18 1·08	42 28 46·3	,,
h	Arg. Z. + 47° − 2625	18 19 0·50	42 29 37·8	,,

Nov. 10. The comet very faint; bright moonlight. The star *a* of the 8th—9th mag.; *b* of the 9th magnitude.

Nov. 17. Comet faint and diffused.

Nov. 27. Comet very faint and diffused.

The observations are corrected for parallax and refraction.

The initials AD., T., and H. are those of Mr. Downing, Mr. Thackeray, and Mr. Hollis.

Royal Observatory, Greenwich:
1883, *Dec.* 14.

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On the Spectrum of Comet b 1883 (*Pons-Brooks*), *observed at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The spectrum of this comet has been examined by Mr. Maunder on the nights of Nov. 27 and Dec. 6 with the "half-prism" spectroscope mounted on the Great Equatorial. The spectroscope was used in the "reversed" position, as in the observations of prominences, so as to give great purity of spectrum. The dispersion from A to H in this position is 5°, and the magnifying power employed was 14.

On Nov. 27 the observations were much interrupted by the passage of light cloud, and no measures were obtained. The spectrum consisted of the three usual cometary bands, with edges bright and sharp towards the red, and fading gradually away towards the blue. No continuous spectrum could be certainly made out, though the band in the yellow, which was very faint, could be traced nearly up to the sharp edge of the band in the green, and that again could be traced very nearly up to the sharp edge of the band in the blue. The bands in the red and violet were not looked for. The band in the green was fairly bright, and the edge towards the red sharp and well defined. The band in the blue was much fainter, and its less refrangible edge not so well defined as that of the band in the green. The band in the yellow was fainter even than that in the blue.

As seen in the finder, the comet appeared nearly circular, about 4′ or 5′ in diameter; it showed a central condensation of stellar appearance, and a faint prolongation of the comet at about 20° position-angle seemed to indicate the commencement of a tail.

On Dec. 6 the comet was decidedly brighter than on Nov. 27, but its appearance as seen in the finder had not undergone any material change. The spectrum, however, seemed somewhat different. There was a very distinct continuous spectrum about 1″ in breadth, and a fainter continuous spectrum was suspected to extend for 25″ or 30″ on both sides of this bright central spectrum. The band in the green was traced for a total length of 4′, and probably extended further still. The band in the yellow was traced for a total length of about 2′ or 2′ 10″; the band in the blue for about 70″ or 80″. The band in the yellow was now very much brighter than that in the blue; the intensities of the three bands being roughly estimated as follows:—Yellow band 5. Green band 10. Blue band 1. The red and violet bands were not looked for. The less refrangible edges of all three bands were very sharp and well defined.

The following measures were obtained of the sharp edge of the green band of the comet spectrum as compared with the

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green band of the spectrum given by a vacuum tube containing alcohol.

Greenwich Sidereal Time.	Comet-band—Alcohol band		Inferred Wave-length of Comet band.
	Micrometer Reading	Wave-length tenth-metres	
h m	r		
0 30	1·885	34·5	5162·5
0 45	2·269	41·5	5155·5
1 5	2·176	39·8	5157·2
1 20	1·980	36·2	5160·8
1 40	1·708	31·3	5165·7
		Mean	5160·3

The green band of the comet spectrum was obviously not coincident with the green band of the alcohol spectrum, but far to the blue of it. There was, however, a bright line in the spectrum from the alcohol vacuum tube coincident with the green band of the Bunsen-flame, and with this the comet-band was very nearly coincident. It seemed, however, a little to the blue of it. The width of the slit during the above measures was 15·8 tenth-metres.

Thalén's wave-length for the less refrangible edge of the green band in the spectrum of the alcohol was assumed throughout: viz. 5197·0 tenth-metres.

Royal Observatory, Greenwich:
1883, *Dec.* 13.

Rotation Period of Jupiter. By W. F. Denning.

Observations of the last few years have proved that one rotation period will not satisfy the motion of different markings on the surface of *Jupiter*. Nor will one period satisfy the same marking for a lengthened interval, as the individual spots have in certain prominent cases shown a slackening motion, the amount of which increases with the time.

At Bristol since 1880, September 27, the great red spot has been observed as it crossed the planet's central meridian on 152 nights. The equatorial white spot has been similarly recorded on 150 nights. Other markings of apparently less permanent character have been carefully watched for shorter intervals. I have partly derived the longitudes of the red and white spots and otherwise reduced the observations by the aid of Mr. Marth's valuable Ephemerides published in the annual volumes of the *Monthly Notices*.

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As to the red spot, the following observations have been obtained during the present apparition:—

	On C. M.	Longitude.
	h m	°
1883, Aug. 23	16 48	67·3
Oct. 15	15 37	71·7
17	17 14	71·1
20	14 44	71·6
Nov. 3	16 17	73·8
18	13 34	72·1
19	19 25	74·8
25	14 25	76·3
29	17 38	75·0
Dec. 4	16 48	77·4
5	12 41	78·6

The observed times of the passages across the central meridian of *Jupiter* are in every case estimated. This method, though undoubtedly less accurate than direct measurement, is found to be very reliable, for the apparent motion of an object near the central meridian is so great that an interval of 3^m is sufficient to introduce a change of place obvious to the eye. It is believed therefore, that in all the observations where the conditions of seeing have been favourable the average error is less than 2^m. The longitudes have been computed from the daily rate of $870°·42$ = rotation of $9^h\,55^m\,34^s·47$, which Mr. Marth has adopted for his recent Ephemerides.

The spot shows a very decided increase in its rotation period. My results for the last few oppositions are as follow:—

Interval.	Number of Rotations.	Period of Rotation.
		h m s
1880, Sept. 27–1881, Mar. 17	413	9 55 35·6
1881, July 8–1882, Mar. 30	640	9 55 38·2
1882, July 29–1883, May 4	674	9 55 39·1
1883, Aug. 23–1883, Dec. 5	251	9 55 38·8

Comparing my observation on 1883, December 5, $12^h\,41^m$, with an observation made by Mr. Trouvelot at Cambridge, Mass., on 1878 September 25, 6^h55^m, Cambridge mean time, I find that the average rotation period of the spot has been

h m
9 55 36·2

during the 1897 days (=4586 rotations) embraced by the observations.

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Professor Hough gives the period as $9^h\ 55^m\ 35^s{\cdot}9$ for the interval 1879, September 25–1882, March 29, comprising 916 days (2214 rotations). He notes that in 1879 the period was $9^h\ 55^m\ 34^s{\cdot}0$, while during the last opposition it had increased to $9^h\ 55^m\ 38^s{\cdot}4$. These results are in very good accordance with those obtained at Bristol.

At the present time the spot follows the assumed first meridian of *Jupiter* (*Monthly Notices*, June 1883) by about $2^h\ 10^m$, and the interval is increasing at the rate of 4^m per month.

During the opposition of 1882–3 the spot became extremely faint, so that when the planet left us in May 1883 it was felt that, should the spot continue to fade, it must cease to be visible by the time *Jupiter* reappeared from conjunction at the end of August. But the spot was re-detected by Mr. A. S. Williams at Brighton, and by myself at Bristol, on the morning of August 24, and since then has been distinguished by several other observers, though Signor Ricco, of the Palermo Observatory, failed to find any traces of the spot on the mornings of September 11, 14, and 21, and announced its disappearance.* With the 10-inch Refractor of that Observatory the region of the disk formerly occupied by the spot appeared very white. Mr. Williams, however, saw the spot well as it came to the central meridian of *Jupiter* on September 14 and 16, and during the ensuing months of October and November it has been very plainly visible. On December 4, as it crossed the central meridian, I saw it with great distinctness and definiteness of outline, and conclude that it is certainly as plain now as it was nine months ago. The fact that it has become more prominently visible during the last few weeks is explained by the more favourable position of *Jupiter*, and not by any actual deepening of tint in the spot itself.

It is altogether open to conjecture whether this marking is identical with certain large and definite spots which have been observed in former times, and exhibited a nearly similar period of rotation. The spots seen by Hooke and Cassini in 1664 and subsequent years cannot be held to have any certain relation to the one now visible, as there is an absence of intermediate evidence to serve as a connecting link. The old observations unmistakably refer to an object in nearly the same position as the spot of our own time, but the former must have been considerably less in magnitude, it being estimated as only one-tenth the diameter of *Jupiter*, whereas the present red spot is about one-third; in fact the latter is fully three times the size of the former.

But though it seems impossible to trace a distinct association with markings which appeared at such a remote period, we may perhaps discern a more satisfactory analogy with some modern observations. We may feel assured, however, that in the time of Lassell and Dawes there was no large and permanent spot on the

* Sig. Ricco writing to *L'Astronomie* for Dec. 1883, states that he succeeded in re-detecting the spot on Sept. 28.

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surface of *Jupiter*. Had such a phenomenon presented itself it must have awakened a keen interest in observers eager to avail themselves of the investigation which such an important apparition must invite. But in 1869 and 1870 an object was discovered by Mr. Gledhill, which from its position, motion, and aspect appears to have been the precursor of the red spot. It was first seen on November 14, 1869, and "carefully observed until March 1870, but in July of the same year it disappeared," though on December 1, 1871, a similar object was observed apparently resting on the southern margin of the equatorial dark belt. The marking seen by Mr. Gledhill was in the form of a dark elliptical ring, its major axis lying parallel with the belts. It was darkest at the E. end, and appears to have occupied the same latitude as the red spot. On January 23, 1870, and other occasions, a narrow belt was noticed immediately outlying the great S. belt, and bent abruptly to the N. just under the following end of the ellipse. This is precisely what has been observed in regard to a small belt following the red spot in 1880 and 1881, and the fact of the dark E. extremity of the ellipse offers additional corroboration to the theory of its identity with the red spot, for the latter during the time I have observed it has always shown a very dark minute spot at its E. end. Moreover, the rotation period of the ellipse during the winter months of 1869–70 was about $9^h\,55^m\,33^s$, which nearly agrees with that of the red spot.

Should this ellipse have developed into the red spot, it is curious what became of it during the years from 1871 to 1878. Mr. Corder in March and April 1872 saw a large elliptical spot in the S. hemisphere of the planet, and Lord Rosse in the following year observed a similar object. In 1876 Mr. Russell at Sydney also mentions having seen the spot, though during the opposition of that year no corroboration is met with amongst the results of other observers.

If the ellipse of 1869–70 was really the forerunner of the red spot, the period of rotation has increased but little during the intervening ten years, and the slackening motion which has become so well defined since 1880 cannot previously have affected the spot to any great extent, unless there is an oscillatory motion of long period, which seems quite possible. In connection with this question it is interesting to note that the increasing retardation of the red spot as observed chiefly in 1881 and 1882 seems now to have become less decided. My own observations show an increase of 2·6 sec. in the rotation period, ascertained during the successive apparitions of 1880–81 and 1881–82, whereas the increase in 1882–3 over 1881–82 was only 0·9 sec. And the observations so far as they have progressed during the present apparition prove that the period has decreased 0·3 sec. as compared with the results of the previous apparition. The question naturally arises, therefore, as to whether the spot has now commenced to drift in W. longitude? If so, we shall soon realise its effects in the shortening of the rotation period. The

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observations of the last few months are very suggestive in this respect.

The equatorial white spot has continued well visible during the present year, though on the whole I believe it is scarcely so brilliant as in the autumnal months of 1880. From a large number of observations obtained here since 1882 I have selected about one per month to exhibit the increasing longitude of the spot relatively to the daily rate of 878°·46=rotation of $9^h\ 50^m\ 7^s{\cdot}42$, which Mr. Marth found conformable to the observations of 1880 Oct.–1882 Aug.:—

	On C. M.	Longitude.
	h m	°
1882, July 29	15 49	29·8
Aug. 23	15 51	29·9
Sept. 27	11 52	28·9
Oct. 25	18 32	31·0
Nov. 26	17 37	31·7
Dec. 23	8 51	32·9
1883, Jan. 23	12 33	42·6
Feb. 23	6 13	41·5
Mar. 24	8 45	45·1
May 4	8 32	45·0
Aug. 24	16 35	62·8
Oct. 15	17 56	67·3
Nov. 3	14 17	64·3
25	17 25	66·2

It is evident by the marked increase of longitude, which the table shows, that this white spot moves considerably slower than during the earlier observations. For the purpose of tracing the extent of this retarded velocity, I have arranged my observations into three periods, with the following result:—

Interval.	Number of Rotations.	Period of Rotation.
		h m s
1880, Oct. 20–1881, Sept. 30	842	9 50 5·8
1881, Sept. 30–1882, Dec. 23	1095	9 50 8·8
1882, Dec. 23–1883, Nov. 25	823	9 50 11·4

The rate of increase in the rotation period during the three years included in the observations has been nearly six seconds, which is greater than the retardation in the motion of the red spot. The mean period of the white spot from the whole interval of my observations is

h m s
9 50 8·7,

which comprises 1131 days (=2760 rotations).

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The conjunctions of the two spots are extremely interesting, because at such a time their great differences of velocity are strikingly manifested. In the *Monthly Notices* for Jan. 1882 I gave the interval of these conjunctions as $44^d\ 10^h\ 42^m$, and from the observations which have accumulated since that time this period is proved to be not very far from the truth. On Nov. 25, at $17^h\ 25^m$ the white spot followed the red spot by 3^h, and on Dec. 19 at $12^h\ 3^m$, the white spot preceded it by $2^h\ 6^m$, so that allowing for the diurnal gain of $13^m\ 24^s$ by the white spot, the conjunction must have occurred (from a mean of the two observations) on Dec. 9 at about 15^h. This compared with an observation on Nov. 19, 1880, at $9^h\ 23^m$, when the two markings occupied exactly the same longitude, gives the average period from 25 conjunctions which have occurred in the interim of $1115^d\ 5^h\ 37^m$ as

d h m
44 14 37·5.

Should the two spots remain visible, they may be observed near conjunction at the following times :—

1884.	h m	1885.	h m
Jan. 23	5 37·5	Jan. 15	2 37·5
Mar. 7	20	Feb. 28	17 15
Apr. 21	10 52·5	Apr. 13	7 52·5
June 5	1 30	May 27	22 30
July 19	16 7·5	July 11	13 7·5
Sept. 2	6 45	Aug. 25	3 45
Oct. 16	21 22·5	Oct. 8	18 22·5
Nov. 30	12 0	Nov. 22	9 0

The motion of the white spot is affected by some curious variations. Even in a few nights it occasionally shows a considerable displacement, so that the resulting longitudes vary in the most striking and apparently irregular manner. Referring to the table of longitudes on the previous page, we shall find that it increased nearly 10° during the month preceding 1883 Jan. 23, and that during the interval from 1883 May 4 to Aug. 24 the increase was nearly 18°.* But during the last few observations, up to 1883 Nov. 25, the velocity of the spot appears to have been more consistent. These singular displacements of position show irregularities both in extent and period, and have been well corroborated by other observers. Thus Prof. Hough ("Report of Dearborn Observatory, 1882," p. 50, remarks, "During 1881 the spot, observed continuously for a period of 252 days, in-

* This displacement of 18° is equivalent to more than 13,000 miles on the surface of *Jupiter*.

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dicated sudden deviations in its apparent place, probably due to change of shape in the object observed." On Oct. 8, 1881, he noted "a maximum displacement of 16 minutes of time, or more than 3″ of arc at mean distance."

There are two other bright objects near the principal white spot, and it has sometimes occurred to me that the rapid displacements in its position may result from the alternate visibility of these several objects. While the chief spot is near its minimum, one of the bordering spots may assume greater prominence, and the observer will thus be easily misled. This explanation offers a satisfactory solution to the phenomena observed; but though I have critically watched the behaviour of these objects I could never be certain that this supplied the real cause of the difficulty. Could their relative appearances be noted nearly every night for a few weeks the point might doubtless be settled.

Apart from its vagaries of motion, the white spot changes its visible aspect very materially. It is sometimes extremely large and brilliant, shining with almost startling effect on the equatorial border of the great southern dark belt. At other times it is very small and faint, and only perceptible after close examination. On four different occasions I have absolutely failed to detect any vestige of the spot, though the central part of the disk was thoroughly examined at the time of its passage.

The spot also varies in form as well as in brightness. It is usually in the shape of a brilliant oval, well defined, and occasionally quite as large as the third satellite of *Jupiter*; but it is sometimes seen as a diffused white patch apparently emerging from the N. edge of the belt. Whenever the spot is bright and well defined there is a trail of light, or diffused luminous matter, running from its east side as though there were an eruption of luculent material from the spot which the rapid rotation of the planet from W. to E. caused to drift in an opposite direction.

The cause of the obscurations * to which the spot is subject would be easily explained on the supposition that the dark spots and patches which abound along the equatorial zone move with a different velocity, and temporarily overlap it. But from careful observation of the equatorial markings it is proved that their

* I may adduce as an example of these variations of light, some observations obtained here in Dec. 1882 and Jan. 1883. On Dec. 22 and 23 the spot was recorded as very brilliant, but on Jan. 1 it had become so far reduced as to be utterly invisible in my 10-inch Reflector. On Jan. 3 I re-detected it, but it was very faint. On Jan. 9 the spot was described as bright, and on the 13th had arrived at a maximum, so as to present a very conspicuous appearance on the disk of the planet. But a far more surprising change was noticed in regard to a small white spot immediately preceding the chief spot (by about 9°) in Nov. 1880. On the 17th, at $7^h\ 56^m$ this object was described as small and bright as it passed the C. M., but on the 19th it was quite obliterated. A few days later, I received a letter from Mr. F. C. Dennett of Southampton, in which he referred to the same thing, so that in this case we have a well-corroborated instance of rapid change.

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velocity is identical with that of the white spot. They are seen to maintain the same relative distances, so that, whatever the cause of the obscurations in the white spot, it is certainly not due, as was formerly supposed, to the interposition of the dark spots in the same latitude. We are thus led to the inevitable conclusion that the variations of brightness are attributable to some inherent phenomena resulting from the peculiar constitution and condition of the spot itself. The appearances would indicate a volcanic nature, and its frequent increase and decrease of brilliancy would seemingly be the index to its alternating periods of activity and quiescence. In any case, it is probable the spot shines with intrinsic light, and it would be an important feature in connection with the progress of these inquiries to witness the transit of a satellite-shadow across it. The projection of such a shadow cone upon the spot might not obliterate it entirely, whence we should infer that its excessive lustre must be due to some other agency than reflection.

The lengthening period is quite opposed to any thesis which attributes to the spot a permanent situation on *Jupiter*. It must in some sense be atmospheric, for whatever it may be, and whatever conditions affect its existence, it cannot be an integral part of the planet, but is more probably a mass or volume of matter quite detached, though formerly evolved, from the surface. The fact that it moves with much greater velocity than the red spot leads us to the inference that, while the latter is on the extreme outer envelopes of the planet, the white spot is amid the lower strata, and rendered conspicuously visible by the convulsions which it is evident are of frequent occurrence on the equator. And one of the most remarkable features of this object is its long duration in a region where many changes are apparently operating to oppose its persistency. We have evidence of this in the different aspect which the spot assumes at short intervals, and in the oscillatory movement to which it is commonly subject.

There can be little question that this particular marking existed for some years before it came under general observation in the autumn of 1880, for the records and drawings of the few preceding oppositions contain obvious reference to an object of precisely similar character and position. Thus Prof. Hough's results at the Dearborn Observatory in 1879 include figures of *Jupiter* on Sept. 20, $9^h\ 45^m$, and Oct. 15, $8^h\ 7^m$, Chicago mean times. In the former of these delineations the white spot is well shown on the extreme west limb of the planet, and in the latter it is seen very slightly in advance of the middle of the red spot, so that a conjunction of the two markings must have occurred a few hours previously. Now, computing back on the period of $44^d\ 14^h\ 37^m{\cdot}5$ we shall find that the present white spot would be in conjunction with the red spot on 1879, Oct 15; hence the absolute identity of the object seen on that date by Prof. Hough with the one now visible appears to be beyond question.

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It was not so regarded by him* however, on account of a marked increase of rotation period which must have taken place during the oppositions of 1879, 1880, and 1881, and moreover between the epoch of 1880 Oct. 1 and Oct. 28, the spot, if the same, must have suffered a great retardation, as it came to the central meridian fully 30 minutes after its computed time. This represents an increase of longitude of about 18° in 27 days, which is rather startling if we admit its actual occurrence. But in view of the "sudden deviations in its apparent place," which Prof. Hough and others have frequently observed, we may well consider it probable that the objects seen by him in 1879 and 1880 were identical with that now visible. In corroboration of this idea we have the conspicuous appearance of the spot detected by him in the earlier months of the series, and the fact that it is the only one depicted in his sketches. Besides, the position of his spot of 1879 indenting the great S. dark belt is exactly the same as that of the spot of 1880, which, from its brilliant appearance and obtrusive position with regard to this belt, could hardly fail to arrest the common attention of observers.

The rotation periods of the equatorial white spots observed by Prof. Hough were as follows :—

Interval.	Period of Rotation.
	h m s
1879, Sept. 10–1880, July 8	9 50 3·5
1880. July 8–1880, Oct. 1	9 50 0·5
1880, Oct. 28–1881, Jan. 30	9 50 9·8
1881, July 22–1882, Mar. 31	9 50 9·8

Prof. Hough would now, however, appear to regard several of these objects as identical, nothwithstanding the marked extension of period, for he says ("Report of Dearborn Observatory, 1883," p. 12), "The equatorial white spot which was first observed in 1879 was again visible during the past opposition [1882–3]. The rotation period as deduced last year, viz. $9^h\ 50^m\ 9^s{\cdot}8$ satisfies the observations."

The increasing period of the white spot (which Professor Hough's observations since 1880 October 28 do not corroborate), representing as it does a considerable retrograde movement on the planet, is very interesting, as the same phenomenon is well defined in regard to the red spot, proving that whatever the origin may be, it operates in both cases, and apparently in nearly similar degree according to my own observations. The further

* He notes: "There are two principal spots, differing in longitude about 15° and in latitude about 1°." If, therefore, the spot observed by him for rotation in 1879 and 1880 is not absolutely the same as the one observed in later years, it must have had a much shorter period than at present, inasmuch as it preserved the same approximate difference in longitude from the similar spot which before 1880 Oct. 1 certainly indicated a period not greater than $9^h\ 50^m\ 3^s{\cdot}5$.

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investigation of the longitudinal drift in these markings and its rate of increase will be an important object of future investigation.

Though I have occasionally observed other spots upon the planet, and recorded their times of transit, they have generally been too evanescent to be followed long. A very curious discordance of periods has been exhibited, but they are none the less certain, and it may be useful to present some of the results in a tabular form to show the differences referred to. The various periods are corrected for equation of light, differences of Jovicentric longitude, and phase :—

Ref.	Interval.	Number of Rotations.	Period of Rotation.
			h m s
a	1878, Sept. 25–1883, Dec. 5	4586	9 55 36·2
b	1880, Oct. 31–1881, Jan. 16	186	9 55 17·9
c	1882, Oct. 30–1882, Nov. 26	66	9 51 47
d	1882, Nov. 1–1882, Nov. 26	61	9 50 17
e	1880, Oct. 20–1883, Nov. 25	2760	9 50 8·7
f	1880, Oct. 29–1881, Jan. 7	171	9 48 0·0
	1880, Oct. 29–1881, Jan. 8	174	9 47 58·2

a. The great red spot in the S. hemisphere.
b. A short dark belt or elliptical spot s.p. the red spot.
c. A bright spot slightly N. of the equator.
d. A dark patch on the equator.
e. The equatorial white spot (slightly S. of the equator).
f. Two dark spots on a belt about 25° N. of the equator. These objects moved with greater velocity than any other markings. Relatively to the red spot they completed a revolution around *Jupiter* in $32^{d}\ 10^{h}$. The rotation period of these spots seems to have been not only less than that of any other markings visible of late years, but also of any markings ever seen on the planet and observed for rotation.

From the above summary it is clear that the periods of different spots vary to the extent of $7^{m}\ 38^{s}$, and it is fair to conclude that these limits admit of considerable increase by further observation. Whether the shorter periods are more nearly representative than the longer ones of the true rotation-period of the planet's sphere must remain a matter of opinion in the absence of more definite evidence on the subject. It seems to me very probable that the real length of the Jovian day is even shorter than that of any period which has been hitherto indicated by the various atmospheric markings. It is more easy to admit that such markings have a tendency to fall into arrear of the great velocity of the globe of *Jupiter* than that objects of such permanency as the white spot have a direct motion of more than 250 miles per hour upon the planet. The behaviour of

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certain spots proves their disposition to lengthen out in longitudinal directions, and it is significant that this extension appears invariably to take place on the following side. The curious offshoot from the great S. dark belt which was seen in the autumn of 1880 N. of the f. end of the red spot developed into a very prominent belt by the constantly increasing prolongation of its E. end. And the dark patches near the equator generally run from the S. equatorial belt in an oblique direction to the E.; in fact all the visible markings seem to be more or less influenced by this tendency. The behaviour of these objects is evidently controlled by the rapid axial rotation of the planet from which they are evolved. And the true period of this rotation is at present unknown, but is probably much shorter than that usually adopted, though it may never admit of accurate determination, owing to the seemingly persistent obscuration of the planet's real surface.

Bristol: 1883, *Dec.* 24.

On Humidity as a Cause of Variation in the Rate of Chronometers.

By Major-General J. F. Tennant, R.E., F.R.S.

(*Extract from a letter to the Secretary.*)

I recently sent a paper on the change of rate of a chronometer which was due to the humidity of the air. Possibly this may be in time to be added as a postscript if the paper be printed in the *Notices*.

The change to dry weather has been accompanied by the corresponding change of rate, that from October 22 to November 1 having been $-3^{s}\cdot41$ (losing). I have also heard from Mr. F. Chambers, at Bombay, who has examined the rates of sixty-three chronometers, and in five cases has found rates which are certainly independent of the temperatures, but which do appear to depend on the humidity. He finds (what my results show, though not conclusively) that the variation increases as time passes. If this be confirmed it is almost certain, I conceive, that the variation of rate is due to a gradual change in the oil.

Calcutta:
1883, *Nov.* 12.

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Ephemeris for finding the Positions of the Satellites of Uranus, 1884. By A. Marth.

The angle of position P of the minor axes, the major and minor semi-axes a and b of the apparent ellipses described by the satellites, and the latitude of the Earth above the assumed plane of their orbits, are the following:—

Greenw. Noon. 1884.	P	*Ariel.* a_1	*Ariel.* b_1	*Umbriel.* a_2	*Umbriel.* b_2	*Titania.* a_3	*Titania.* b_3	*Oberon.* a_4	*Oberon.* b_4	Latit. of Earth.
	°	″	″	″	″	″	″	″	″	°
Jan. 9	285·59	14·76	+ 3·11	20·56	+ 4·33	33·72	+ 7·11	45·09	+ 9·51	+ 12·17
19	·59	14·88	3·11	20·74	4·34	34·01	7·11	45·48	9·51	12·07
29	·60	15·00	3·09	20·90	4·30	34·28	7·05	45·84	9·43	11·87
Feb. 8	·60	15·10	3·04	21·04	4·23	34·51	6·94	46·16	9·28	11·60
18	·60	15·19	2·97	21·16	4·13	34·70	6·78	46·41	9·07	11·27
28	·60	15·25	2·88	21·24	4·01	34·84	6·58	46·59	8·80	10·89
Mar. 9	·59	15·28	2·78	21·29	3·87	34·92	6·33	46·69	8·48	10·47
19	285·59	15·29	+ 2·66	21·30	+ 3·71	34·93	+ 6·09	46·72	+ 8·14	+ 10·04
29	·58	15·27	2·55	21·27	3·55	34·89	5·83	46·65	7·79	9·61
Apr. 8	·57	15·22	2·44	21·21	3·40	34·78	5·57	46·51	7·45	9·21
18	·57	15·15	2·33	21·11	3·25	34·62	5·33	46·30	7·13	8·85
28	·56	15·06	2·24	20·98	3·12	34·42	5·12	46·03	6·84	8·55
May 8	·55	14·96	2·16	20·83	3·01	34·17	4·94	45·70	6·61	8·32
18	·54	14·84	2·11	20·67	2·93	33·90	4·81	45·33	6·43	8·16
28	285·54	14·70	+ 2·07	20·49	+ 2·88	33·61	+ 4·73	44·94	+ 6·32	+ 8·09

The longitudes, $u-U$, of the satellites reckoned in their orbits from the points which are in superior conjunction with the planet's centre, are approximately the following:—

Greenw. Noon. 1884.	*Ariel.* u_1-U	*Ariel.* Diff.	*Umbriel.* u_2-U	*Umbriel.* Diff.	*Titania.* u_3-U	*Titania.* Diff.	*Oberon.* u_4-U	*Oberon.* Diff.
	°	°	°	°	°	°	°	°
Jan. 9	184·90	1428·46	31·86	868·74	26·71	413·53	221·03	267·39
19	173·36	·43	180·60	·73	80·24	·52	128·42	·38
29	161·79	·41	329·33	·71	133·76	·50	35·80	·37
Feb. 8	150·20	·38	118·04	·68	187·26	·48	303·17	·36
18	138·58	·36	266·72	·66	240·74	·47	210·53	·35
28	126·94	·33	55·38	·65	294·21	·46	117·88	·34
Mar. 9	115·27	·30	204·03	·64	347·67	·46	25·22	·34
19	103·57	·28	352·67	·62	41·13	·45	292·56	·33
29	91·85	·26	141·29	·60	94·58	·45	199·89	·33
Apr. 8	80·11	·24	289·89	·60	148·03	·44	107·22	·34
18	68·35	·23	78·49	·60	201·47	·45	14·56	·34
28	56·58	·23	227·09	·61	254·92	·46	281·90	·35
May 8	44·81	·22	15·70	·60	308·38	·47	189·25	·36
18	33·03	1428·22	164·30	868·61	1·85	413·47	96·61	267·37
28	21·25		312·91		55·32		3·98	

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These values are to be interpolated for the times for which the positions of the satellites are required. The position-angles p and distances s are then to be found by means of the formulæ—

$$s \sin (p-\mathrm{P}) = a \sin (u-\mathrm{U})$$
$$s \cos (p-\mathrm{P}) = b \cos (u-\mathrm{U}).$$

The satellites move in the direction of *in*creasing position-angles, and will be at their greatest elongations ("N" in posit. P+90° and "S" in posit. P−90°) and at their superior and inferior conjunctions with the planet about the following Greenwich mean times :—

Ariel.

1884.	N.	S.		N.	S.		N.	S.
	h	h		h	h		h	h
Jan. 8	8·1	9 14·3	Feb. 25	5·3	26 11·6	Apr. 13	2·7	14 8·9
10	20·5	12 2·8	27	17·8	29 0·0	15	15·1	16 21·4
13	9·0	14 15·3	Mar. 1	6·3	2 12·5	18	3·6	19 9·9
15	21·5	17 3·8	3	18·8	5 1·0	20	16·1	21 22·4
18	10·0	19 16·2	7	7·3	7 13·5	23	4·6	24 10·9
20	22·5	22 4·7	8	19·8	10 2·0	25	17·1	26 23·4
23	11·0	24 17·2	11	8·2	12 14·5	28	5·6	29 11·9
25	23·5	27 5·7	13	20·7	15 3·0	30	18·1	
28	11·9	29 18·2	16	9·2	17 15·5	May		2 0·4
31	0·4		18	21·7	20 4·0	3	6·6	4 12·9
Feb.		1 6·7	21	10·2	22 16·5	5	19·1	7 1·3
2	12·9	3 19·2	23	22·7	25 4·9	8	7·6	9 13·8
5	1·4	6 7·6	26	11·2	27 17·4	10	20·1	12 2·3
7	13·9	8 20·1	28	23·7	30 5·9	13	8·6	14 14·3
10	2·4	11 8·6	31	12·2		15	21·1	17 3·3
12	14·9	13 21·1	Apr.		1 18·4	18	9·6	19 15·8
15	3·3	16 9·6	3	0·7	4 6·9	20	22·1	22 4·3
17	15·8	18 22·1	5	13·2	6 19·4	23	10·6	24 16·8
20	4·3	21 10·6	8	1·7	9 7·9	25	23·1	27 5·3
22	16·8	23 23·1	10	14·2	11 20·4	28	11·6	29 17·8

Umbriel.

	N.	S.		N.	S.		N.	S.
	h	h		h	h		h	h
Jan. 9	16·1	11 17·8	Feb.		1 11·1	Feb. 24	6·1	26 7·8
13	19·5	15 21·2	3	12·8	5 14·5	28	9·6	
17	23·0	20 0·7	7	16·3	9 18·0	Mar.		1 11·3
22	2·4	24 4·2	11	19·7	13 21·4	3	13·0	5 14·8
26	5·9	28 7·6	15	23·2	18 0·9	7	16·5	9 18·2
30	9·3		20	2·6	22 4·4	11	20·0	13 21·7

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1884.	N. h	S. h		N. h	S. h		N. h	S. h
Mar. 15	23·4	18 1·2	Apr. 13	23·7	16 1·4	May 8	20·5	10 22·3
20	2·9	22 4·6	18	3·2	20 4·9	13	0·0	15 1·7
24	6·4	26 8·1	22	6·7	24 8·4	17	3·5	19 5·2
28	9·8	30 11·6	26	10·1	28 11·9	21	6·9	23 8·7
Apr. 1	13·3	3 15·0	30	13·6		25	10·4	27 12·1
5	16·8	7 18·5	May		2 15·3	29	13·9	31 15·6
9	20·4	11 22·0	4	17·1	6 18·8			

Titania.

Super. Conj.	h	N. Elong.	h	Infer. Conj.	h	S. Elong.	h
Jan. 8	8·5	Jan. 10	12·7	Jan. 12	17·0	Jan. 14	21·2
17	1·4	19	5·7	21	9·9	23	14·1
25	18·4	27	22·6	30	2 8	Feb. 1	7·1
Feb. 3	11·3	Feb. 5	15·6	Feb. 7	19·8	10	0·0
12	4·3	14	8·5	12	12·7	18	17·0
20	21·2	23	1·5	25	5·7	27	9·9
29	14·2	Mar. 2	18·4	Mar. 4	22·7	Mar. 7	2·9
Mar. 9	7·2	11	11·4	13	15·6	15	19·9
18	0·1	20	4·4	22	8·6	24	12·9
26	17·1	28	21·3	31	1·6	Apr. 2	5·8
Apr. 4	10·1	Apr. 6	14·3	Apr. 8	18·6	10	22·8
13	3·0	15	7·3	17	11·5	19	15·8
21	20·0	24	0·3	26	4.5	28	8·8
30	13·0	May 2	17·2	May 4	21·5	May 7	1·7
May 9	6·0	11	10 2	13	14·4	15	18·7
17	22·9	20	3·2	22	7·4	24	11·7
26	15·9	28·	20·1	31	0·4	June 2	4·6

Oberon.

Super. Conj.	h	N. Elong.	h	Infer. Conl.	h	S. Elong.	h
Jan. 0	17·6	Jan. 4	2·4	Jan. 7	11·2	Jan. 10	20·0
14	4·7	17	13·5	20	22·3	24	7·1
27	15·9	31	0·7	Feb. 3	9·4	Feb. 6	18·2
Feb. 10	3·0	Feb. 13	11·8	16	20·6	20	5·4
23	14·2	26	23·0	Mar. 1	7·8	Mar. 4	16·6
Mar. 8	1·4	Mar. 11	10·2	14	19·0	18	3·7
21	12·5	24	21·3	28	6·1	31	14·9
Apr. 3	23·7	Apr. 7	8·5	Apr. 10	17·3	Apr. 14	2·1
17	10·9	20	19·7	24	4·5	27	13·3
30	22·1	May 4	6·9	May 7	15·7	May 11	0·5
May 14	9·3	17	18·1	21	2·9	24	11·6
27	20·4	31	5·2	June 3	14·0	June 6	22·8

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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W. H. M. CHRISTIE, M.A., F.R.S., Vice-President, in the Chair.

William Henry Davies, 55 Great Newton Street, Liverpool;
Edward Henry Nightingale, M.A., 3 Park Row, Albert Gate, S.W.;
Cuthbert E. Peek, M.A., 7 Portland Place, W.; and
The Rev. W. H. K. Soames, M.A., Chesterton Road, Cambridge;

were balloted for and duly elected Fellows of the Society.

Note giving a Numerical Illustration of the Effects of a Percentage Change on the Sun's Mean Motion in Longitude. By E. J. Stone, M.A., F.R.S.

The following is a direct numerical proof of the changes which must result from any alteration of the values of the sidereal times at mean noon adopted for the determination of the so-called mean solar times which are used for the comparison of any existing Lunar and Planetary Tables with observations.

I shall suppose a change of one per cent. to be made in the adopted value of the Sun's mean motion in longitude. This will render the effects due to the change distinctly visible in a few days, without affecting the generality of the reasoning. The effects due to the change made in 1864 can then be inferred directly by a simple proportion.

Now, taking the Greenwich mean noon of the *Nautical Almanac*, 1883 Dec. 1, as our epoch, and expressing our time,

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in terms of the adopted day we shall have, according to Le Verrier's Solar Tables

(1) Sun's mean longitude $= l = 249° 59' 55''{\cdot}22 + 3548''{\cdot}330235 \cdot t$,

from which we find by the usual formula for the Right Ascension of the Greenwich meridian

(2) $S = 16^h\ 39^m\ 59^s{\cdot}68 + (24^h + 3^m\ 56^s{\cdot}555349)t +$ Nutation;

and let us assume that by the adoption of the value of l given in (1) we have found for the longitude of the Moon's centre of gravity

$$L = A + Nt,$$

and with the times t, deduced in the usual way from the adoption of the expression

$$S = 16^h\ 39^m\ 59^s{\cdot}68 + 3^m\ 56^s{\cdot}555349 \times t + \text{Nutation}$$

as the sidereal times at mean noon, we have found

$$A = 268° \text{ and } N = 47436''{\cdot}72653,$$

so that

(3) $$L = 268° + 47436''{\cdot}72653 \times t.$$

But if we adopt for the Sun's mean longitude

(4) $$l = 249° 59' 55''{\cdot}22 + 3512''{\cdot}84693268 \times t',$$

we should find from the usual formula for S

(5) $S' = 16^h\ 39^m\ 59^s{\cdot}68 + (24^h + 3^m\ 54^s{\cdot}18979551) \times t' +$ Nutation,

and, although I will not assume it, from the necessary proportionality of the real motions of L, S, and l, however the units of time be changed, we should have

(6) $$L = 268° + 46962''{\cdot}3592647 \cdot t'.$$

The values of S and S′ at tabular mean noons, when $24^h \times t = 24^h \times t' = 24^h \times$ same integer, are given in the following table under the heads of A and B respectively.

Date.		Green. Mean Noon *N.A.* A	Tabular Mean Noon. B
		h m s	h m s
1883, Dec.	1	16 40 0·18	16 40 0·18
	2	16 43 56·74	16 43 54·37
	3	16 47 53·29	16 47 48·56
	4	16 51 49·85	16 51 42·75
	5	16 55 46·41	16 55 36·95
	6	16 59 42·96	16 59 31·13
	7	17 3 39·52	17 3 25·33
	8	17 7 36·08	17 7 19·52
	9	17 11 32·63	17 11 13·71
	10	17 15 29·19	17 15 7·90
	11	17 19 25·75	17 19 2·09

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The results given under A are identical, as they should be, with those of the *Nautical Almanac*. Now, if we could assume, consistently with the conditions imposed upon us by the absolute necessity of (1), (2), and (3) representing the *same* real and independent motions before and after the change of the variable,

$$t = t'(1 - 0\cdot000027304334),$$

it would be found that S, which does equal

$$16^h\ 39^m\ 59^s\cdot68 + (24^h + 3^m\ 56^s\cdot555349)t + \text{Nutation},$$

would become

$$16^h\ 39^m\ 59^s\cdot68 + (24^h + 3^m\ 54^s\cdot1897955\,1)t' + \text{Nutation},$$

or S would be identical with S′, if the substitution is one consistent with the physical conditions of our problem.

It is upon the identity of S and S′ with this small change of t that all the opposition to the acceptance of my views is in reality based; and this is the supposed practical disproof of my results.

But if S does equal S′, then, as we are concerned with the mean longitude of the *real* Sun, and not with the longitude of an arbitrary mean Sun, we ought to have $l = l'$, which is not the case.

$$\begin{aligned}\text{For } l \text{ which} &= 249^\circ\ 59'\ 55''\cdot22 + 3548''\cdot330235 \times t\\ &= 249^\circ\ 59'\ 55''\cdot22 + 3548''\cdot330235\,.\,t'(1 - 0\cdot000027304334)\\ &= 249^\circ\ 59'\ 55''\cdot22 + 3548''\cdot233350\,.\,t.\end{aligned}$$

or expressed in time $= 16^h\ 39^m\ 59^s\cdot68 + 3^m\ 56^s\cdot548890\,.\,t'$, and consequently, therefore, we shall have

		h	m	s
Dec. 1.	R.A. of meridian at tabular mean noon	= 16	39	59·68
	Mean longitude of Sun in time at — ditto —	= 16	39	59·68
	Difference ...	= 0	0	0·00
Dec. 11.	R.A. of meridian at tabular mean noon	= 17	19	1·58
	Mean longitude of Sun at — ditto =	= 17	19	25·14
	Difference ...	= 0	0	23·56

and *as we have made the positions of the meridian agree whether we are using t times or t′ times by the relationship*

$$t = t'(1 - 0\cdot000027304334),$$

the adjustment of the Sun to the meridian cannot be made by shifting the position of the meridian, and must therefore be due to an erroneous allowance for the Sun's motion; a result which is

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directly indicated by the differences between the values of l and l'.

Conversely, if we attempt to transfer the expressions (4) and (5) by assuming that

$$t' = t(1 + 0{\cdot}0000273),$$

into the expressions (1) and (2), it will be found that although the values of S will agree, the values of the Sun's mean longitude will disagree; and the same thing will be brought out by appealing to the longitudes of the Moon.

I have already given in my papers the explanation of these difficulties. They are due to the neglect of the unit of time T in the equation of condition by which we have eliminated ω from our theoretical investigations, on the assumption that

$$\omega = 360 \times 60 \times 60'' + n, \text{ instead of } \frac{360 \times 60 \times 60}{T} + n.$$

The existence of the factor T leads at once to the necessity of a correction $-0^{h}{\cdot}24 \times t'$, before the results of Table B can replace those of Table A.

I am afraid that Prof. Adams has hardly sufficiently considered the fact that although the rotation of the Earth rapidly destroys angular errors and thus leads to small corrections of time when we determine the time directly from observations; yet these small corrections are not effective in removing the discordances between the tabular places and the observations. The tabular R.A. of the Moon from Hansen's Tables are now nearly a second of time too large. If we were to determine the clock errors from the tabular R.A. of the Moon instead of from our sidereal observations, we should only, therefore, deduce a correction of about a second in time to the adopted G.M.T. But this correction of time would not destroy the discordance between the tabular place of the Moon and observation. It is impossible to play fast and loose with the meridian in this way. The rapid motion of the Earth is as effective in displacing the relative positions of the meridian and any object in a given time δt as it is effective in destroying a given angular error in the time δt.

With Prof. Cayley's views I quite agree, except that, as a matter of fact, we do not at present make any distinction between the real Sun's mean longitude and that of the longitude of the mean Sun; and that we cannot therefore admit such distinctions in practice. If we did it would lead to my results.

For when we adopted with my notation α_1, we should have $n_1 = \alpha_1 + x_1$, and when we adopted α_2 we should have $n_2 = \alpha_2 + x_2$, and the relation between x_1 and x_2 would not be $\alpha_2 - \alpha_1$, since I have shown it to be impossible that $n_2 - n_1$ should equal $\alpha_2 - \alpha_1$, when we are dealing with the Sun's *real* motion in longitude. We cannot measure motion accurately in mean solar time so long as mean solar time is itself variable. And the corrections required for the breaks of continuity must be consistent with the founda-

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tions of our theoretical investigations, that in each unit of time the Sun's mean longitude did increase some definite and assigned angle, or these breaks of continuity will destroy all existing tables and introduce apparent inequalities not due to gravitation.

Note on Professor Adams's paper in the Monthly Notices for December 1883. By E. J. Stone, M.A., F.R.S.

Professor Adams's argument respecting the small difference between Bessel's and Le Verrier's sidereal times at mean noon is sufficiently treated on pages 36 and 37. It is impossible to assume, as Professor Adams does, that the meridian can have two directions in space at the same instant of absolute time.

Professor Adams's argument, page 46, line 11, that "mean solar time is measured, not by the Sun's mean motion in longitude, as Mr. Stone's theory supposes, but by the motion of the Sun in hour angle," is one which I do not profess to understand. However the mean solar time is determined, the tabular motion in longitude must represent the true motion of the Sun in longitude, if the tables are to agree with observations. That I, therefore, do not allow an arbitrary change in the adopted values of the sidereal times at mean noon to change the tabular motion in longitude, whilst Prof. Adams confessedly allows such changes to be introduced, appears to prove that I am right and Prof. Adams is wrong.

On page 46 Professor Adams proves correctly enough that with my a_1 and a_2 he has—

$$\frac{T_1 - T_2}{T_2} = \frac{a_1 - a_2}{360^\circ + a_2}, \quad \text{or} \quad = \frac{\delta n}{360^\circ + n}$$

with Professor Adams's notation, but it is impossible that both a_1 and a_2 or n and $n + \delta n$ can represent the real motion of the Sun in longitude in the units T_1 and T_2. (See page 34.) My result on this point is confirmed by Professor Cayley, who gets my result if the expression for the mean longitude of the sun is any continuous function. (See page 48.)

It is true that mean time can be determined from the time of the Sun's transit and the equation of time. The error in time thus determined will not, however, remove the errors of the tables in longitude. It merely expresses the time which the meridian and not the Sun takes in moving from the tabular position to the true position, and such corrections can therefore only remove the $\frac{1}{366}$th part of tabular errors due to arbitrary changes in the adopted R.A. of meridian at mean noons. Take for example:—

On 1881 September 16 the observed R.A. of the Sun on the Greenwich meridian, with error of sidereal clock found from stellar observations, was $11^h\ 40^m\ 33^s{\cdot}05$, whilst the tabular R.A.

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was $11^h\ 40^m\ 33^s{\cdot}09$. The tabular longitude is therefore too large by $0''{\cdot}60$ according to this observation, when the clock errors are determined in the usual way. But if we determine the error of the sidereal clock, not from the stellar observations, but from the tabular R.A., which is the method mentioned by Professor Adams as that of the application of the equation of time, we shall find a sidereal time at noon larger by $0^s{\cdot}04$ than that found from the stellar observations. The difference of time thus found is, as Professor Adams remarks, very small; but the motion of the Sun in $0^s{\cdot}04$ is only about $0''{\cdot}00164$ in longitude; and this is the only effect produced by the change of time upon the tabular error of longitude, and is exactly the $\frac{1}{366}$th part of that required to remove the tabular error $0''{\cdot}60$.

Professor Cayley should certainly accept my results, for his conclusion is that I am right unless there are two mean Suns available (page 48, 7 lines from bottom); and Prof. Adams on page 44 distinctly states, "The only mean Sun known to astronomers is an imaginary body, which moves uniformly in the equator at such a rate that the difference between its Right Ascension and that of the true Sun consists wholly of periodic quantities." I agree that this is our present usage.

In conclusion, I merely call attention to the fact that in my work I use tabular days T_1 and T_2, such that Sun's true mean longitude $= A + n_1 t_1 = A + n_2 t_2$, where n_1 and n_2 are assigned angles, and T_1 T_2 are the corresponding adopted units. The advantage of this method is that we can correctly allow for the change of unit due to a change from n_1 to n_2 in the adopted value of the Sun's mean motion in longitude, for

$$\frac{T_1 - T_2}{T_2} = \frac{n_1 - n_2}{n_2}$$

is determinate. Attempts to measure motion in fallible units of the time used as our independent variable cannot but lead to error.

Remarks on Major-General Tennant's paper "On the Change in the adopted Unit of Time." By Prof. J. C. Adams, M.A., F.R.S.

The December number of the *Monthly Notices* contains a paper by Major-General Tennant in which the author arrives at conclusions which appear to him to confirm Mr. Stone's views respecting a change in the unit of mean solar time. In reality, however, those conclusions are quite consistent with my own as given in the same number of the *Monthly Notices*, and not at all with Mr. Stone's.

According to Major-General Tennant (*Monthly Notices*, p. 43), the factor by which the tabular mean motions should be multiplied in consequence of the change from Bessel's to Le Verrier's determination of the ratio of the mean solar to the sidereal day is what he calls

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$$\frac{\text{Sidereal Seconds in Le Verrian Mean Day}}{\text{Sidereal Seconds in Besselian Mean Day}}.$$

Now, if n be the Sun's mean motion in a mean solar day as determined by Bessel, the sidereal seconds in a mean solar day will be

$$86400 \times \frac{360^\circ + n}{360^\circ}.$$

But if $n+\delta n$ be the Sun's mean motion in a mean solar day as determined by Le Verrier, the sidereal seconds in a mean solar day will be

$$86400 \times \frac{360^\circ + n + \delta n}{360^\circ},$$

and therefore the factor above referred to by Major-General Tennant will be

$$\frac{360^\circ + n + \delta n}{360^\circ + n} = 1 + \frac{\delta n}{360^\circ + n},$$

whereas, according to Mr. Stone's views, this factor should be

$$\frac{n + \delta n}{n} = 1 + \frac{\delta n}{n},$$

where the difference from 1 is nearly 366 times greater than it should be.

The same thing may be otherwise shown thus:—

If N denote the number of mean solar days in a mean tropical year, according to Bessel's determination, then N+1 will be the corresponding number of sidereal days in the same interval.

Consequently, the ratio of the length of a mean solar to that of a sidereal day will be

$$\frac{N+1}{N} = 1 + \frac{1}{N}.$$

But if $N+\delta N$ denote the number of mean solar days in a mean tropical year, according to Le Verrier's determination, then $N+\delta N+1$ will be the corresponding number of sidereal days in the same interval.

And consequently the above-mentioned ratio will become

$$\frac{N+\delta N+1}{N+\delta N} = 1 + \frac{1}{N+\delta N}$$

Hence the ratio of the length of a mean solar to that of a sidereal day will be changed in the ratio of

$$\frac{1 + \frac{1}{N+\delta N}}{1 + \frac{1}{N}} = 1 - \frac{\delta N}{N(N+1)}, \text{ nearly,}$$

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whereas, according to Mr. Stone, the ratio which measures this change would be

$$\frac{N}{N+\delta N} = 1 - \frac{\delta N}{N}, \text{ nearly,}$$

where, as before, the difference from 1 is nearly 366 times too great.

Mr. Stone's error appears to arise from his *equating* two things which are really different, and which are inconsistent with each other,—viz. Bessel's and Le Verrier's determinations of the Sun's mean motion in longitude in the same interval of time.

Major-General Tennant is wrong in supposing that solar observations are no longer employed in Observatories for the determination of mean solar time. If this were the case, it would only show that the Observatories had taken a very retrograde step, since the final test whether the mean solar times have been correctly found can only be supplied by solar observations. Whenever the mean solar times are deduced from the observed sidereal times, it is tacitly assumed that the tabular mean longitudes of the Sun which have been employed are correct; and if this is not the case, the mean solar times deduced will require a corresponding correction, which can only be found by solar observations.

Thus mean solar time may be determined with reference to a natural phenomenon,—viz. the transit of the true Sun over the meridian of a given place; and the mean solar day is the average of all the apparent solar days defined as the intervals between two successive transits, and therefore has nothing arbitrary about it. To speak of Besselian mean time and Le Verrian mean time, or of the Besselian mean solar day and the Le Verrian mean solar day, can produce nothing but confusion in our ideas of the measure of time.

Additional Note on the Change in the Unit of Time.

By Prof. A. Cayley, M.A., F.R.S.

Bessel's mean Sun is what Bessel supposed the mean Sun to be: that is, a point in the heavens the mean longitude of which is

$$l = \text{const.} + 1296027''{\cdot}618184 \,.\, t + \text{term in } t^2,$$

the longitude being measured from what Bessel supposed to be the mean equinoctial point, and the unit of time being what Bessel supposed to be the Julian year of 365·25 mean solar days.

Similarly Hansen's mean Sun is what Hansen supposed the mean Sun to be: that is, a point in the heavens the mean longitude of which is

$$l = \text{const.} + 1296027''{\cdot}674055 \,.\, t + \text{term in } t^2,$$

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the longitude being measured from what Hansen supposed to be the mean equinoctial point, and the unit of time being what Hansen supposed to be the Julian year of 365·25 mean solar days.

Mr. Stone, as I understand him, contends that for the same instant of time the two points are one and the same point. If so, it seems to me that Hansen's formula must be a mere analytical transformation of Bessel's, and the question would arise, Why was any change made? My contention is that the points are not one and the same point; and in support I show that, assuming them to be so, we obtain two inconsistent values for the time of the Earth's rotation. As a theoretical definition of the mean Sun, I agree that "The only mean Sun known to astronomers is an imaginary body which moves uniformly in the equator at such a rate that the difference between its Right Ascension and that of the true Sun consists wholly of periodic quantities." But in astronomy there is no such thing as absolute accuracy; and I contend that neither Bessel's mean Sun nor Hansen's mean Sun is absolutely identical with the mean Sun as thus defined.

The Belts of Saturn. By Henry Pratt.

Inquiry was made at the last meeting by Mr. Ranyard whether a narrow belt on *Saturn* had been seen by others than himself. And from the reports of the meeting in the *Observatory* and *Astronomical Register* I have no doubt he referred to a narrow belt which I also saw on several occasions in October, while making observations of the inner satellites; and again on Nov. 6 and 28, while preparing to photograph the planet, I repeated the view of the same object; but it was only to identify it as a portion of features familiar to me for many years.

For the sake of clearness, I shortly describe the whole belt system, so far as it is rendered by my With speculum of 8·15-in. aperture.

The belts of the two hemispheres are, in general arrangement, usually similar and symmetrical. On both sides of the equator similar alternations of colour and proportion are usually seen. For some distance on each side of the equator extends a zone which is usually free from any markings whatever, showing only a creamy-yellow tint. At a latitude of, roughly, about 10° occurs a strong belt, narrow, sharply defined on its equatorial side, but diffused on its polar side, and gathered in places into wispy knots and curved markings. The colour of this I always see as Vandyke brown. This narrow belt is the deepest tint of all the markings on the globe; and when our atmosphere is not perfectly translucent, it is almost or quite the only detail which can be seen. So far as I can understand, this narrow belt was the subject of discussion at the last meeting. The polar side of this brown belt and its wispy curves is softened into a narrow

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zone of cream tint, which again merges into one of very pale rose-madder, which ends in a moderately sharp boundary, about latitude 25°. This is followed by a zone a few degrees in width, and of cream tint; and about latitude 40° commences a double belt, very pale rose-madder in tint, the space between them being of cream tint, and the belt highest in latitude being the broadest of the two. In turn this is followed by a zone of cream tint until the polar cap of bluish-white is reached.

Of course, in speaking of colours only faint tints are to be understood throughout the above description. I did not see the whole of these features during October, simply because I did not look for them all, but on three occasions I observed the greater portion of the detail mentioned, and by that means ensured the correct identification of the narrow belt to which Mr. Ranyard referred.

The photograph * accompanying this paper illustrates the belt system described, and is a reduced copy of a water-colour drawing which I made at my telescope on 1874 Sept. 14; and on numberless occasions since then I have re-observed the same features.

18 *Preston St., Brighton:*
1884, *Jan.* 10.

Note accompanying a Sketch of the Great Comet (b) 1882.

By Gen. G. H. Willis.

(*Extract from a letter to Mr. Stone.*)

I send you the sketch of the appearance of the comet of last year on the morning of October 19.

The air was extremely clear at the time, and the wind was nearly calm.

I watched the comet for some twenty minutes, until the increasing light of dawn obliterated the dark circle round it. During this time I got my field-glasses to observe with, but I found that the dark part was not so distinct through them. I then got a powerful ship's telescope, but with it the dark shade almost entirely disappeared, except on the right, or darker, side, where it remained slightly visible.

The appearance was so extremely delicate, light, and airy that it would be almost impossible to depict it on paper.

41 *Eaton Place, S.W.:*
1883, *Nov.* 15.

* The Photograph is placed in the Society's Library.

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From a sketch by Gen. G.H. Willis made at 5 a.m. on Oct. 19, 70 miles East of Gibraltar.

W.H Wesley lith. West Newman & Co imp.

THE GREAT COMET (b) 1882.

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Observations of Comet III. 1882 (Barnard's). By W. H. Finlay, B.A.

I made three observations of this comet at Aberdeen Road (South Africa), where I was stationed for the Transit of *Venus*, and as they are of later date than any I have seen published they may be useful. No micrometers were provided for the 6-inch Equatorials sent out by the Transit Committee, but I fitted a ring-micrometer (belonging to the Cape Observatory) to mine. The longitude of my station was determined by exchanges of telegraphic signals with the Cape Observatory on four nights, and the latitude by transits of stars over the prime vertical on two nights. The results are:—

$$\lambda = -1^h\ 37^m\ 15^s{\cdot}6. \qquad \phi = -32^\circ\ 45'\ 57''.$$

The observations of the comet are corrected for refraction, and the diameter of the ring was determined by observations at the time. The comet presented the appearance of a small round diffused mass, slightly more condensed towards the centre, which was the point observed.

Date.		Local Sid. Time.	$d\alpha\,(\delta-*)$	d N.P.D. $(\delta-*)$	No. of Transits.	Comp. star.
		h m s	m s	′ ″		
1882, Dec.	3	1 16 33·8	−1 7·54	−12 19·4	6	*a*
	5	1 51 0·4	−0 15·35	+ 1 25·9	3	*b*
	7	1 54 27·4	−2 22·44	−12 33·5	7	*c*

The stars *a* and *b* have each been observed twice on the meridian at the Cape Observatory, and the results placed at my disposal by Mr. Gill. Star *c* is number 9214 of Mr. Stone's Catalogue. Their mean places for 1882·0 and corrections to date of observation are:—

Star.	Mag.	Right Ascension.	Corr.	N.P.D.	Corr.
		h m s	s	° ′ ″	″
a	8	16 29 28·42	+3·74	150 55 7·91	+0·08
b	9	16 38 36·17	+3·76	150 15 8·86	−0·65
c	6½	16 49 37·64	+3·76	149 8 30·29	−1·41

and the resulting places of the comet are:—

Date.		Greenw. M.T.	Right Ascension.	Parall. Factor.	N.P.D.	Parall. Factor.
		h m s	h m s		° ′ ″	
1882, Dec.	3	6 49 20·1	16 28 24·62	+0·086	151 7 27·4	+0·75
	5	7 15 49·2	16 38 24·58	+0·077	150 13 42·3	+0·78
	7	7 11 23·8	16 47 18·96	+0·070	149 21 3·4	+0·81

Royal Observatory, Cape of Good Hope:
1883, *Nov.* 24.

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The Elements of the Orbit of Comet a, 1883 (*Brooks–Swift*). By Robert Bryant.

The following observations of this comet were made

	App. R.A.	App. δ
	h m s	° ′ ″
1883, Mar. 3·29997 G.M.T.	0 12 23·663	+32 0 47·27

at Cambridge, England; and

		App. R.A.	App. δ
	h m s	h m s	° ′ ″
Mar. 23	7 43 50 Paris M.T.	2 51 58·5	+25 27 33
Apr. 12	8 27 36 Paris M.T.	4 19 24·2	+15 56 12

at the Paris Observatory.

These observations were corrected for the effects of parallax and aberration by means of approximate elements previously determined; and then by the variation of curtate distances parabolic elements were determined. The following elements, referred to the mean equinox 1883, are those which gave the least residuals in the middle place.

$$T = 1883, \text{ Feb. } 18{\cdot}95224 \text{ G.M.T.}$$
$$\pi - \Omega = 110^\circ\ 56'\ 29''{\cdot}2$$
$$\Omega = 278\ 5\ 59{\cdot}5$$
$$i = 78\ 5\ 37{\cdot}7$$
$$\log q = 9{\cdot}8809143. \quad \text{Motion direct.}$$

These elements give for the errors of the middle place (Observation − Computation)

$$\Delta\alpha \cos\beta = +34''{\cdot}1, \qquad \Delta\beta = +74''{\cdot}0.$$

where α and β denote the geocentric longitude and latitude respectively.

The interval between the extreme observations is 40 days, and the arc of anomaly described 51°.

The Meteor Shower of Pons's Comet. By W. F. Denning.

The meteoric radiant point of Pons's comet (1812) is at 200°+68°·5 December 6+, and the theoretical meteor speed is 37 miles per second.* The comet, however, in crossing the node, is some 20 millions of miles distant from the earth's orbit on the in-

* British Association Report on Luminous Meteors, 1875, p. 234. The radiant point computed from MM. Schulhof and Bossert's orbit (1884) is at 197°·8+67°·3 (*Nature*, Jan. 17. p. 273).

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terior side, so that the possibility of a rencontre with any meteoric fragments belonging to it is very questionable.

In 1876, at the end of November and early part of December, I discovered a feeble star-shower proceeding from a point 6° N. of *α Draconis*. I recorded 5 of these meteors on the evening of December 8. The radiant point was at 208°+71° (*Monthly Notices*, vol. xxxvii. p. 111, Radiant No. 44). The correspondence of this shower both in date and position with the cometary radiant as above is very close, but the display, as I observed it, merely represented one of those attenuated streams which abound in every region of the heavens. The individual meteors were slow-moving, star-like bodies, devoid of streaks or trains, whereas the meteors of Pons's comet should have a velocity very nearly as great as the Perseids. It is extremely doubtful, therefore, whether this shower of Draconids, seen in 1876, can have any physical relation with the comet of Pons; though the accordance may be worth mentioning. The number of known meteoric systems encountered by the Earth is now so great that agreements of this kind carry little weight with them unless they are of a very marked and significant character.

During the last few years I have made no observations at the particular epoch of these Draconids, but on December 8, 1883, I casually saw a bright, white meteor in the fore part of *Ursa Major*, which, from its foreshortened track, obviously belonged to this radiant N. of *α Draconis*.

Bristol: 1884, *Jan.* 2.

Spectroscopic Results for the Motions of Stars in the Line of Sight obtained at the Royal Observatory, Greenwich, in the year 1883. No. VII.

(*Communicated by the Astronomer Royal.*)

The results here given are in continuation of those printed in the *Monthly Notices*, vol. xxxvi. p. 318, vol. xxxvii. p. 22, vol. xxxviii. p. 493, vol. xli. p. 109, vol. xlii. p. 230, and vol. xliii. p. 81. The observations were made with the "half-prism" spectroscope, one "half-prism" with a dispersion of about 18½° from A to H being used, except in a few cases of bright stars, mentioned in the remarks, where a train of two "half-prisms" with a dispersion of 80° from A to H was used. An eyepiece with a magnifying power of 14 was employed throughout.

The cylindrical lens has always been used in front of the slit as in the observations made previously to 1881. A slip of metal coated with Balmain's luminous paint inserted immediately behind the measuring pointer has been frequently employed to give a phosphorescent illumination of the field.

The observations of the Moon and of the sky spectrum have been made as a check on the general accuracy of the results.

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Motions of Stars in the Line of Sight, in Miles per Second, observed with the Half-prism Spectroscope.

(+ denotes Recession; − Approach)

The initials M and N are those of Mr. Maunder and Mr. Nash respectively.

Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M. per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks.
				α Andromedæ.			
Aug. 23	N	2	F	−10·8	−23·1	−21·8	Spectrum bright.
Oct. 17	M	3	F	+ 3·4	−21·7	−20·9	Definition poor.
				β Cassiopeiæ.			
Nov. 16	M	3	F	+ 4·2	+ 5·9	+ 6·8	Spectrum bright & steady.
				γ Cassiopeiæ.			
Nov. 16	M	3	F	+ 2·6	+10·9	+10·5	Spectrum rather tremulous.
				α Arietis.			
Nov. 10	M	3	F	+ 3·9	+10·5	+12·7	Star-line rather faint.
20	N	2	b_1	+ 7·0	− 0·5	− 0·9	Spectrum bright but tremulous.
				β Persei.			
Nov. 16	M	3	F	0·0	−48·0	−45·9	Star-line dark.
				α Persei.			
Nov. 16	M	2	F	− 1·7	−26·1	−29·0	Star-line ill-defined.
				Aldebaran.			
Oct. 17	M	2	F	−12·8	+63·2	+65·1	Definition poor.
Nov. 10	M	2	F	− 6·3	+38·5	+36·6	
12	M	2	F	− 5·7	+32·6	+30·5	Spectrum very faint.
20	N	2	b_1	− 3·2	+34·6	+37·9	
23	M	2	b_1	− 2·2	+ 6·0	+ 7·0	Spectrum very tremulous.
Dec. 5	N	2	F	+ 1·8	+28·0	+25·3	Spectrum faint.
				Capella.			
Sept. 18	N	2	F	+16·7	+ 0·8	− 1·4	Sky hazy.
Nov. 6	N	2	F	−10·1	+40·5	+39·8	
12	M	2	F	− 8·6	+28·7	+36·1	Spectrum faint.
20	N	2	b_1	− 6·4	+44·3	+47·2	Spectrum bright & steady.
23	M	3	b_1	− 5·6	+52·2	+40·7	Definition very bad.
Dec. 5	N	2	F	− 2·0	+50·9	+42·8	Spectrum very bright.

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Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M. per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks
				Rigel.			
Feb. 23	M	2	F	+15·8	+22·6	+22·6	Star-line faint.
Nov. 16	M	2	F	− 5·7	+27·0	+27·1	Spectrum tremulous.
				β Tauri.			
Nov. 12	M	2	F	− 9·5	+12·9	+15·7	Spectrum faint.
				α Orionis.			
Nov. 23	M	2	b_1	− 7·8	+33·7	+43·2	Definition bad.
				β Aurigæ.			
Nov. 12	M	2	F	−10·9	+16·2	+19·2	Spectrum very faint.
				Sirius.			
Feb. 16	M	4	F	+10·3	+10·2	+16·3	Definition poor.
23	M	4	F	+11·4	− 0·7	− 1·8	Spectrum bright.
Mar. 30	M	4	F	+14·2	− 4·5	− 3·2	Two-prism train.
30	M	6	F	+14·2	−19·3	−19·6	
Nov. 16	M	4	F	−10·8	− 6·0	− 5·6	Direct comparison showed a distinct displacement towards the blue.
Dec. 5	N	2	F	− 7·0	− 1·1	+ 0·2	Star-line very broad.
				Castor.			
Feb. 16	M	3	F	+11·8	+11·4	+30·0	Definition fair.
				Procyon.			
Feb. 16	M	2	F	+10·1	+20·9	+32·1	Definition good.
23	M	2	F	+11·8	+16·1	+17·0	Star-line well seen.
Mar. 30	M	2	F	+17·2	− 3·2	− 5·0	Definition fair.
30	M	2	F	+17·2	+ 2·1	+ 2·4	Two-prism train. Spectrum too faint for this dispersion.
				Pollux.			
Feb. 16	M	2	F	+10·8	−49·3	−71·2	Definition fair.
23	M	2	F	+12·5	−33·1	−31·7	Star-line very faint.
Mar. 30	M	2	F	+17·8	−45·0	−41·2	Star-line indistinct.
May 16	M	3	F	+14·9	−66·9	−51·9	Spectrum tremulous.
Nov. 20	N	2	b_1	−15·2	−13·7	−17·5	Spectrum bright.
23	M	2	b_1	−14·6	−18·6	−24·1	Spectrum very tremulous.

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Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M. per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks.
				Regulus.			
Feb. 23	M	2	F	+ 2·2	+23·7	+25·9	Star-line fairly distinct.
Mar. 30	M	2	F	+12·3	+18·1	+14·7	Definition good.
May 23	N	2	F	+17·8	− 8·4	− 8·0	Spectrum very tremulous.
23	M	2	F	+17·8	+ 5·9	+ 6·5	
31	N	1	F	+17·5	− 6·6	− 0·7	Spectrum faint.
				β Ursæ Majoris.			
July 13	M	2	F	+ 5·6	− 5·3	− 5·6	Spectrum tremulous.
				β Leonis.			
May 23	M	2	F	+16·7	+ 4·3	+ 7·6	Star-line not well defined.
				γ Ursæ Majoris.			
July 13	M	2	F	+ 7·4	+ 1·6	+ 5·3	Spectrum tremulous.
Aug. 21	N	2	F	0·0	+53·3	+55·6	Spectrum bright.
				ε Ursæ Majoris.			
July 13	M	2	F	+ 7·5	+ 2·1	+ 5·2	Definition good.
Aug. 21	N	2	F	+ 1·6	+24·8	+23·4	Star-lines very distinct.
				α Canum Venaticorum.			
June 1	M	2	F	+13·4	+11·8	+ 9·3	Spectrum faint, but star-line distinct.
				α Virginis.			
Mar. 30	M	2	F	− 3·9	− 3·5	+ 4·2	Definition good.
May 7	M	2	F	+ 7·6	−55·6	−50·5	Definition very bad.
16	M	3	F	+ 9·9	−26·3	−24·5	Spectrum bright but tremulous.
24	M	2	F	+11·8	−51·0	−52·0	Observations made with difficulty.
31	N	2	F	+13·3	−19·8	−22·7	
				ζ Ursæ Majoris.			
June 1	M	2	F	− 2·2	+25.4	+24·9	Spectrum fairly bright.
July 13	M	2	F	+ 7·8	+24·7	+27·7	Definition good.
				η Ursæ Majoris.			
May 21	M	1	F	+ 9·5	− 3·1	− 3·5	Spectrum faint.
23	M	4	F	+ 9·6	+12·4	+14·7	Spectrum bright & steady.
July 13	M	2	F	+ 9·3	−21·2	−23·3	Star-line indistinct Observations unsatisfactory.
Aug. 13	M	2	F	+ 5·9	− 3·5	− 1·8	

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Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M. per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks.
				Arcturus.			
Mar. 30	M	2	F	− 3·4	−35·6	−30·8	Definition good.
May 7	M	2	F	+ 6·4	−54·1	−43·5	Spectrum tremulous.
21	M	2	F	+ 9·5	−49·0	−45·7	Star-line fairly well seen.
23	M	2	F	+ 9·9	−34·1	−34·2	Spectrum very bright.
28	M	2	b_1	+10·8	−69·4	−68·5	Star-line well defined.
June 1	M	2	F	+10·2	−60·3	−55·6	Spectrum bright.
4	M	2	b_1	+12·0	−47·1	−51·7	Spectrum bright & steady.
8	N	2	b_1	+12·6	−68·0	−57·1	Spectrum not very bright.
14	N	2	b_1	+13·4	−67·5	−65·2	
26	N	2	F	+14·6	−30·1	−30·6	Star-line distinct.
July 4	M	3	b_1	+15·0	−52·2	−54·7	Spectrum tremulous.
5	N	2	b_1	+15·1	−59·8	−57·9	
6	M	3	b_1	+15·1	−64·5	−64·4	Definition fair.
6	M	2	b_1	+15·1	−44·8	−47·4	Two-prism train. Definition poor.
9	M	2	b_1	+15·2	−54·1	−54·3	Spectrum tremulous.
13	M	2	F	+15·3	−54·6	−40·7	Spectrum tremulous.
17	N	2	F	+15·3	−58·0	−58·2	Spectrum very tremulous.
Aug. 13	M	2	F	+13·6	−46·6	−44·8	Spectrum tremulous.
21	N	2	F	+12·5	−46·0	−49·6	Star-line faint.
24	M	1	D_2	+12·0	−89·1	−79·9	Spectrum very tremulous and lines ill-defined.
24	M	1	D_1	+12·0	−44·0	−45·8	
				ϵ^2 *Boötis.*			
May 28	M	3	b_1	+ 8·9	−12·5	−14·9	Star spectrum very faint.
June 4	M	2	b_1	+10·0	−17·0	−20·3	Star-line very difficult to see.
July 5	N	2	b_1	+13·1	−53 6	−50·9	Definition very bad.
				β *Libræ.*			
May 24	M	2	F	+ 4·8	−33·4	−31·3	Star-spectrum & line faint.
31	N	2	F	+ 6·7	−25·9	−27·7	Spectrum faint.
				α *Coronæ Borealis.*			
May 16	M	2	F	+ 3·4	+28·3	+33·1	Spectrum steady. Definition fair.
23	M	2	F	+ 4·8	+12·4	+15·5	Spectrum steady and fairly bright.
Aug. 13	M	2	F	+12·6	+18·9	+18·6	Spectrum very tremulous.

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Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks.
				α Serpentis.			
July 6	M	2	b_1	+13·0	+27·5	+32·8	Star-line faint.
				β Herculis.			
July 6	M	2	b_1	+ 9·2	−45·8	−50·3	Star-line faint & difficult.
				ζ Herculis.			
July 6	M	2	b_1	+ 7·4	−25·2	−25·0	Star-line difficult.
				α Herculis.			
May 28	M	2	b_1	− 1·8	−38·0	−43·6	Measures unsatisfactory.
				α Ophiuchi.			
May 23	M	2	F	− 4·5	+17·6	+17·1	Spectrum bright & steady.
24	M	2	F	− 4·3	− 9·7	− 6·9	Spectrum faint.
				γ Draconis.			
July 4	M	2	b_1	+ 1·3	+14·3	+14·5	Definition fair.
				α Lyræ.			
May 16	M	2	F	− 6·3	−43·8	−42·3	Definition very good.
24	M	2	F	− 5·5	−38·5	−37·1	Definition poor.
31	N	2	F	− 4·7	−18·1	−16·3	
June 26	N	2	F	− 1·3	−22·1	−22·7	
July 19	N	2	F	+ 1·9	−36·4	−42·0	
26	N	2	F	+ 2·9	−24·6	−27·5	
Aug. 3	M	2	F	+ 3·9	−37·8	−25·8	Spectrum faint.
14	N	2	F	+ 5·2	−51·5	−51·7	Spectrum tremulous.
23	N	2	F	+ 6·2	−37·5	−33·4	
Sept. 18	N	2	F	+ 8·2	−23·9	−23·1	
20	N	3	F	+ 8·3	−48·3	−45·2	Spectrum very steady.
Nov. 10	M	2	F	+ 7·3	−34·1	−36·4	Spectrum faint.
Dec. 5	N	1	F	+ 4·5	−23·9	−20·8	Spectrum very faint & tremulous.
				ζ Aquilæ.			
Aug. 14	N	2	F	+ 8·0	−26·9	−19·6	Spectrum faint & tremulous.
				δ Cygni.			
Nov. 16	M	4	F	+ 8·0	−17·3	−17·9	Spectrum faint & tremulous.

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Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M. per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks.
				α Aquilæ.			
July 19	N	3	F	− 0·8	+21·3	+20·0	
26	N	2	F	+ 1·0	− 6·5	+ 4·9	Star-line faint.
Aug. 3	M	2	F	+ 3·0	+15·3	+13·4	Spectrum faint.
23	N	2	F	+ 7·9	−23·0	−22·4	Spectrum bright.
Sept. 18	N	2	F	+13·1	−17·4	−13·1	Spectrum bright.
Oct. 13	M	2	F	+15·9	+ 5·7	+ 4·7	Spectrum faint.
22	M	3	F	+16·2	− 2·6	− 2·9	
Nov. 16	M	3	F	+15·0	− 5·0	− 5·1	Spectrum very tremulous.
				γ Cygni.			
July 4	M	2	b_1	− 6·3	− 4·2	− 4·2	Definition poor.
Nov. 10	M	2	F	+10·1	− 5·3	− 6·0	Spectrum very faint.
				α Cygni.			
June 14	N	3	b_1	− 8·4	+28·8	+27·1	Spectrum bright.
July 4	M	2	b_1	− 6·9	−27·5	−32·5	Star-line faint.
5	N	3	b_1	− 6·8	−41·9	−41·9	Definition poor.
Oct. 22	M	1	F	+ 7·7	−41·4	−39·1	Spectrum faint.
Nov. 10	M	2	F	+ 9·1	−56·4	−50·4	Spectrum bright.
				ε Cygni.			
July 5	N	2	b_1	− 7·8	−59·8	−60·2	Spectrum bright.
Nov. 10	M	2	F	+12·1	+ 9·5	+ 8·6	Spectrum faint.
				α Cephei.			
Nov. 16	M	2	F	+4·6	−39·1	−44·0	Spectrum tremulous.
				ε Pegasi.			
Nov. 10	M	2	F	+17·0	+16·4	+13·3	Star-line very faint.
				Fomalhaut.			
Sept. 20	N	1	F	+ 7·3	−43·4	−46·7	Spectrum faint.
				α Pegasi.			
July 26	N	3	F	−12·5	+13·0	+12·5	Star-line faint.
Aug. 16	N	2	F	− 8·0	+ 5·6	+ 2·6	Spectrum faint.
23	N	3	F	− 6·2	+ 1·9	+ 6·2	Star-line faint.
Oct. 13	M	2	F	+ 8·3	−39·0	−39·7	Spectrum very faint.
17	M	2	F	+ 9·4	−21·6	−20·7	Measures very satisfactory.

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Date.	Obs.	No. of Meas.	Line.	Motion Measured.
		Sky.		
1883, June 6	M	5	b_1	− 3·4
July 4	M	5	b_1	+ 3·8
7	M	5	b_1	− 6·3
Aug. 25	M	4	D	0·0
Nov. 7	N	3	F	+ 1·1
27	M	5	b_1	− 1·7
		Moon.		
Jan. 15	M	3	F	+ 3·4
Feb. 16	M	5	F	− 0·7
23	M	5	F	− 1·0
May 15	N	4	F	− 3·0
16	M	5	F	+ 0·6
21	M	5	F	− 0·8
22	N	4	F	− 1·7
23	M	5	F	− 0·7
24	M	5	F	+ 1·0
June 14	N	2	b_1	− 1·1
26	N	2	F	− 2·2
July 13	M	5	F	+ 0·2
17	N	2	F	− 5·9
19	N	4	F	− 6·1
26	N	2	F	− 1·3
Aug. 13	M	3	F	+ 2·1
14	N	2	F	− 1·2
16	N	2	F	− 3·0
21	N	4	F	− 3·5
23	N	2	F	− 2·9
Sept. 18	N	2	F	− 2·5
20	N	2	F	+ 0·9
Oct. 13	M	3	F	+ 0·7
17	M	5	F	− 2·3
Nov. 10	M	5	F	− 0·5
12	M	5	F	− 0·8
16	M	5	F	+ 2·1
20	N	3	b_1	− 4·9

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Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, in the year 1883.

(*Communicated by the Astronomer Royal.*)

Occultations of Stars by the Moon.

Day of Obs.		Phenomenon.	Telesc.	Power.	Moon's Limb.	Mean Solar Time of Observation.	Obs.
1883.						h m s	
Jan.	20	(*a*) Disapp. 71 Orionis	Altaz.	100	Dark	9 21 55·75	H.
Feb.	16	(*b*) Disapp. χ^2 Orionis	„	100	„	5 59 4·82	L.
	16	(*c*) Disapp. χ^2 Orionis	E. Eq.	140	„	5 59 7·15	H.
Mar.	12	(*d*) Disapp. *o* Arietis	„	60	„	6 51 3·74	L.
	12	Disapp. *o* Arietis	Altaz.	100	„	6 51 3·51	H.
July	17	Disapp. Piazzi xvii. 304	„	100	„	11 30 31 99	H.
Aug.	21	(*e*) Reapp. *e* Piscium	E. Eq.	—	„	12 43 2·27	T.
	24	Reapp. Piazzi iii. 87	Altaz.	100	„	11 21 29·40	A.P.
Sept.	14	(*f*) Disapp. c^1 Capricorni	„	100	„	8 43 42·82	J.P.
	14	Disapp. c^2 Capricorni	„	100	„	8 47 38·48	J.P.
	14	(*g*) Reapp. c^1 Capricorni	„	100	Bright	9 7 0·90	J.P.

Notes.

(*a*) Observation difficult; star disappeared very near the bright limb.
(*b*) „ cloudy, star very faint.
(*c*) Misty; star very faint. (*d*) Observation considered very good.
(*e*) Cloudy. (*f*) Star followed to bright edge; observation difficult.
(*g*) Moon's limb very tremulous.

Phenomena of Jupiter's Satellites.

Day of Observ.		Sat.	Phenomenon.	Telesc.	Power.	Mean Solar Time of Observ.	Mean Solar Time of N.A.	Obs.
1883.						h m s	h m s	
Jan.	4	I.	Ecl. R. First seen	E. Eq.	140	10 24 50	10 24 47	H.
			Full brightness	„	„	10 26 51		
		III.	First seen	„	„	11 42 51	11 45 8	„
			Full brightness	„	„	11 45 58		
	10	(*a*) II.	Tr. Egr. Bisection	„	„	7 58 9	7 56 0	„
	18	I.	Occ. D. First contact	„	„	11 10 35		
			Bisection	„	„	11 13 24	11 16 0	„
			Last contact	„	„	11 16 30		
		(*b*) III.	Occ. D. First contact	„	„	13 55 43		
			Last contact	„	„	14 0 15	14 3 0	„

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Day of Observ.	Sat.	Phenomenon.	Telesc.	Power.	Mean Solar Time of Observ.	Mean Solar Time of N.A.	Obs.
1883.					h m s	h m s	
Jan. 18	I.	Ecl. R. First seen	E. Eq.	140	14 15 24	14 15 18	H.
		Full brightness	„	„	14 17 4		
27	I.	Ecl. R. First seen	Altaz.	100	10 40 28	10 39 33	B.
Feb. 5	III.	Tr. Ing. First contact	E. Eq.	310	10 51 31	10 58 0	A.D.
		Bisection	„	„	10 57 0		
		Last contact	„	„	11 4 59		
9	III.	Ecl. R. First seen	Altaz.	100	7 50 22	7 52 23	B.
10	I.	Occ. D. First contact	E. Eq.	140	11 3 20	11 9 0	H.
		Last contact	„	„	11 7 53		
16	III.	Occ. R. Bisection	„	„	7 0 0	7 3 0	„
		Last contact	„	„	7 3 12		
		Ecl. D. Last contact	„	100	9 15 30	9 13 20	„
	(*c*)	Ecl. R. First seen	Altaz.	100	11 52 15	11 54 13	L.
		First seen	E. Eq.	140	11 52 18		H.
	II.	Occ. D. First contact	Altaz.	100	11 53 20	11 58 0	L.
		Last contact	„	„	11 57 35		„
		First contact	E. Eq.	140	11 53 31		H.
		Bisection	„	„	11 55 58		„
		Last contact	„	„	11 59 53		„
23	III.	Occ. D. First contact	„	310	7 58 0	8 8 0	A.D.
		Bisection	„	„	8 3 19		
		Last contact	„	„	8 8 40		
	III.	Occ. R. First seen	„	„	10 45 8	10 52 0	„
		Last contact	„	„	10 50 52		
	III.	Ecl. D. Last seen	„	„	13 16 13	13 14 14	T.
26	I.	Occ. D. First contact	„	„	9 19 40	9 21 0	L.
		Last contact	„	„	9 21 57		
28	I.	Ecl. R. First seen	„	60	7 20 48	7 20 9	A.P.
Mar. 6	I.	Tr. Ing. Last contact	E. Eq.	140	8 31 58	8 30 0	H.
	(*d*)	Tr. Egr. First contact	„	„	10 44 49	10 46 0	„
		Bisection	„	„	10 46 25		
	(*e*) II.	Ecl. R. First seen	„	100	11 32 24	11 31 24	„
30	I.	Ecl. R. First seen	„	310	9 32 17	9 32 20	L.
		Full brightness	„	„	9 33 20		
31	III.	Ecl. D. Last seen	„	„	9 15 49	9 16 41	A.P.
Apr. 6 (*f*)	I.	Occ. D. First contact	Altaz.	100	7 56 56	8 0 0	A.D.
		Last contact	„	„	8 0 10		

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Day of Observ. 1883.		Sat.	Phenomenon.	Telesc.	Power.	Mean Solar Time of Observ. h m s	Mean Solar Time of N.A. h m s	Obs.
Apr. 25		III.	Tr. Egr. First contact	E. Eq.	140	9 49 29	9 56 0	H.
			Last contact	,,	,,	10 0 27		
May 13	(*g*)	III.	Occ. R. First contact	Altaz.	100	8 47 22		L.
			Last contact	,,	,,	8 54 41		,,
			First seen	E. Eq.	140	8 48 12	8 52 0	H.
	(*h*)		Bisection	,,	,,	8 50 51		,,
			Last contact	,,	,,	8 54 17		,,
	16 (*i*)	II.	Occ. D. First contact	,,	,,	8 56 33	9 7 0	A.D.
			Last contact	,,	,,	9 2 32		
Oct. 16	(*k*)	II.	Occ. R. First seen	,,	310	12 12 41		
			Bisection	,,	,,	12 15 46	12 4 0	L.
			Last contact	,,	,,	12 19 15		
Nov. 6		I.	Ecl. D. Last seen	,,	60	12 49 8	12 49 7	J.P.
		II.	Ecl. D. Last seen	,,	,,	14 25 26	14 26 6	,,
	13	I.	Ecl. D. First contact	,,	140	14 41 51	14 42 12	H.
			Last seen	,,	,,	14 42 58		
	17 (*l*)	II.	Occ. R. Bisection	,,	,,	11 42 43	11 39 0	,,
			Last contact	,,	,,	11 46 10		
	19	III.	Tr. Ing. First contact	Simms Eq.	—	11 16 54		
			Bisection	,,	—	11 19 57	11 24 0	,,
			Last contact	,,	—	11 24 22		
	23	I.	Tr. Egr. Clear of Planet	E. Eq.	—	11 46 37	11 47 0	T.
Dec. 1	(*m*)	I.	Occ. R. First seen	,,	60	10 45 15	10 45 0	L.
			Full brightness	,,	,,	10 47 30		
	14	III.	Ecl. D. First contact	,,	70	8 33 2	8 36 24	H.
			Last seen	,,	,,	8 37 57		

Notes.

(*a*) Cloudy at first and last contact. (*b*) *Jupiter's* limb ill-defined, boiling. (*c*) Observation doubtful: perhaps a few seconds late. (*d*), (*e*) Clouds passing. (*f*) *Jupiter* very tremulous. *g*) First contact doubtful; last considered very good. (*h*) Observation doubtful. (*i*) Observation uncertain; planet low down; *Jupiter's* limb boiling. (*k*) *Jupiter's* limb very badly defined. (*l*) Observation uncertain; *Jupiter's* limb boiling; clouds passing. (*m*) *Jupiter* very faint; sky hazy.

The clear aperture of the object-glass of the E. Equatorial is $6\frac{3}{4}$ inches, of the Altazimuth 4 inches, and of the Simms Equatorial 6 inches.

The initials A. D., T., L., H., J. P., A. P., and B., are those of Mr. Downing, Mr. Thackeray, Mr. Lewis, Mr. Hollis, Mr. Power, Mr. Pead, and Mr. Bennett.

Royal Observatory, Greenwich:
1884, *Jan.* 10.

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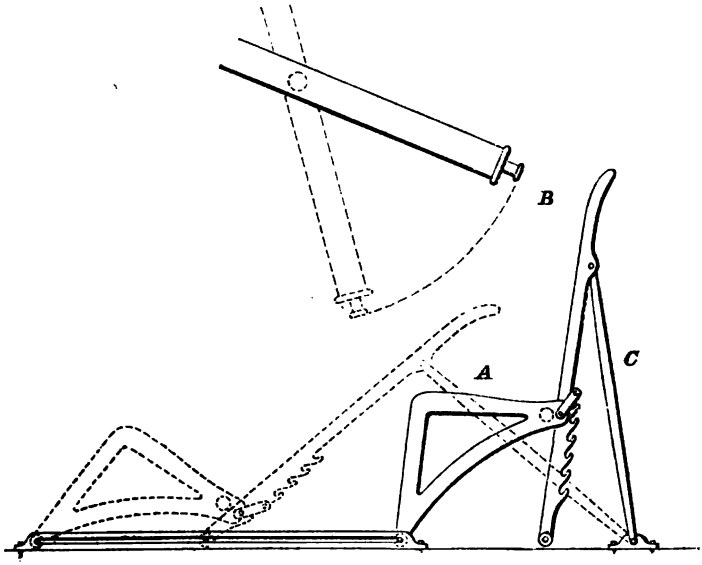
Suggestion for an Observing Seat for Transit Instruments.
By Richard Inwards.

The accompanying drawing shows the proposed form of seat. It consists mainly of three moveable parts—the seat A, the back B, and the strut C, the latter being attached to a hinge-piece screwed into the floor.



The seat is raised or depressed by altering the position of the supporting link upon the rack of B. This at the same time alters the height of the seat and the slope of the back, as well as thrusts the whole chair forward by the action of the hinged strut C, as shown by the dotted lines in the diagram. This enables the observer to follow with comfort all the usual movements of the telescope. The addition of a small moveable pad or cushion behind the head may be necessary to complete the adjustment in some positions. A roller keeps the seat and back always in contact.

A seat on a similar principle can be made in which the observer can by means of a tangent screw and rack alter his position without leaving the seat. A model illustrating this was exhibited to the meeting.

1884, *Jan.* 11.

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On the Latitude of the Observatory, Windsor, N.S. Wales.

By John Tebbutt.

The latitude hitherto adopted (namely, $-33^\circ\ 36'\ 28''\cdot9$) is that of the prime vertical pier of the old Observatory as deduced from prime vertical observations with a 2-in. Transit instrument during the years 1867, 1868, 1869, 1870, and 1874. The stars employed were nine in number, culminating within 39′ of the zenith. Their positions, however, rested almost wholly on observations with the old Parramatta Transit Circle at the Sydney Observatory, and were therefore very imperfect. Of the ninety-three observations fifty-nine were made on the middle wire only of the Transit instrument, and thirty-four on a side wire with the instrument reversed, according to Struve's method, both before and after the meridian passage. I have recently investigated a correction to this latitude from more accurate places of the stars as obtained from the Sydney Catalogues for 1877 and 1878, the Melbourne Catalogue for 1870, the Cape Catalogue for 1880, and recent MS. observations kindly communicated by Messrs. Ellery and Russell. The new result is $-33^\circ\ 36'\ 29''\cdot1$, and the reduction $-0''\cdot4$ applied to this value gives $-33^\circ\ 36'\ 29''\cdot5$ as the latitude of the piers of the Meridian Transit and Equatorial instruments in the new Observatory adjoining, erected in 1879. During the year 1881 I made a new investigation of the latitude by means of an excellent 3-in. Transit instrument temporarily mounted on the prime vertical pier of the new Observatory, the local times being obtained for the purpose by means of the old 2-in. Transit. The stars selected for observation were Nos. 3908, 3916, and 4180 of the Washington Catalogue for 1860, and Nos. 4015, 4309, and 879 of the B.A. Catalogue for 1850. The mean places were adopted from the Southern Catalogues above-mentioned and MS. results obtained by Messrs. Ellery and Russell in 1881, and were weighted according to the number of observations from which each was derived. Proper motion was applied from the best authorities available. The transit system of the 3-in. telescope consists of five threads, and so far as the first five stars are concerned the method adopted was to observe the transits east and west across as many threads as possible, the rotation axis of the instrument being in the same position throughout the observations of each evening. The pivots were, however, reversed between the observations of different evenings. In the case of the last star the transits were observed four times each evening across one or more of the side threads according to Struve's method described in Chauvenet's "Spherical and Practical Astronomy" (1874, vol. ii., p. 262). A value of the latitude was then deduced from each pair of observations of a thread with the pivots in the same direction, and it was thus assumed that the collimation error, which was itself very small throughout the series of observations, would be eliminated from the mean result. During the observations of B.A.C. 879 the opportunity was also taken to note the transit across the middle

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Date of Observations.	Star.	Mean Place of Star 1881·0. R.A.	Declin.	Authorities.	Number of Separate Results. Clamp N.	S.	Concluded Seconds of Latitude.
1881.		h m s	° ′ ″				″
Apr. 4, 6, 7, 8, 11, 12, 13, 14, 15, 16, 19, 20	W.C. 3908	9 13 47·83	−33 35 42·3	Sydney, MS. Obs. 1881, Melb. MS. Obs. 1881, & Cape Cat. 1880	14	14	30·8
Apr. 4, 6, 7, 8, 9, 11, 12, 13, 14, 15, 16, 19, 20	W.C. 3916	9 14 51 87	−33 36 0·9	Sydney MS. Obs. 1881, Melb. MS. Obs. 1881, & Cape Cat. 1880	15	15	31·1
Apr. 7, 8, 11, 12, 13, 14, 15, 16, 19, 20	W.C. 4180	9 56 48·87	−33 36 4·4	Melb. MS. Obs. 1881, & Cape Cat. 1880	10	14	30·8
May 4, 6, 9, 10, 11, 12, 13, 14	B.A.C. 4015	11 46 53·98	−33 14 45·6	Melb. Cat. 1870 Cape Cat. 1880	18	16	31·0
May 9, 10, 11, 12, 13, 14	B.A.C. 4309	12 44 13·95	−33 21 0·2	Sydney Cat. 1877 Melb. Cat. 1870 Cape Cat. 1880	14	14	31·0
Dec. 10, 17, 21, 23	B.A.C. 879	2 44 6·65	−32 54 22·4	Sydney Cat. 1878 Cape Cat. 1880	8	8	30·6

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thread in opposite positions of the rotation axis as described on pages 263 and 264 of the work above cited. I may remark that the equatorial distances of the side threads from the middle thread had been very carefully determined, and that the level employed was a sensitive one of about 1130 feet radius, each division being equal to 0″·8. The individual results throughout the series were highly satisfactory. The table on p. 102 exhibits an abstract of the work on each star.

Three observations of B.A.C. 879 on the middle thread as previously described gave a result of 30″·5. The mean of all the determinations gives −33° 36′ 30″·9 as the latitude of the instrument, to which if we apply the reduction +0″·1 we get −33° 36′ 30″·8 as the latitude of the piers of the Meridian Transit and Equatorial instruments of the new Observatory. This value, which differs 1″·3 from that obtained by means of the old Transit and an imperfect level, will in future be adopted as the geographical latitude of the Observatory.

Observatory, Windsor, N.S. Wales:
1883, *Nov.* 27.

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*Note on M. Loewy's Communication referring to an Apparatus for the Determination of Flexures. With some Remarks on Professor Houzeau's (and Mr. Schaeberle's) Proposal on the same Subject.** By A. Marth.

Before submitting to the critical consideration of readers some remarks on M. Loewy's "Réponse," &c., *Monthly Notices*, XLII. p. 287 ff., I may perhaps be allowed to quote the words in which M. Loewy expressed his opinion on my old proposal in his paper, "Nouvelle Méthode pour Déterminer la Flexure des Lunettes," published in the *Comptes Rendus* of December 9, 1878, vol. lxxxvii. p. 891, so that readers may form their own judgment upon the objections, which, as I stated at the last December (1881) meeting, have been such a puzzle to me.

"M. Marth, astronome anglais, a proposé d'établir, dans le cube central de la lunette, et à peu près sur l'axe de rotation, un

* The present paper (which is that alluded to on p. 349 of vol. xlii.) was intended to be published in the Supplementary Number of vol. xlii. of the *Monthly Notices*. But when I learnt that M. Loewy had already sent a second "Réponse," I thought it better to postpone the publication of my own paper, in order to give him precedence, and to avoid cross-writing. Owing to temporary absence in South Africa, I did not get a sight of M. Loewy's second paper till the end of December, when I saw the Supplementary Number at the Cape Observatory. That second "Réponse" made me the more desirous that interested readers, for whose critical examination I had asked already (*vide* p. 349 of vol. xlii.), might form their own judgment before they read what I may have to say, and the delay will have given them time to do so. Under the circumstances I have thought it better to make some alterations, and to indicate my own opinion rather more plainly than I had first intended.

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appareil se composant de deux objectifs, dont chacun a la moitié de la distance focale de l'instrument, et entre lesquels se trouve placé un miroir argenté percé à son centre.

"Théoriquement, on pourrait arriver à évaluer le déplacement de l'objectif par rapport à un point du réticule, mais il faudrait, pour cela, recourir à des hypothèses qui, dans la réalité, ne peuvent être nullement confirmées. Il faut supposer que le miroir et les deux objectifs se trouvent, dans la rotation de la lunette, reliés ensemble d'une manière invariable, condition presque impossible à réaliser; il faut faire abstraction de l'effet de torsion que produit sur l'appareil la flexion du cube central auquel il est attaché et tenir compte de l'effet de sa propre pesanteur. On voit immédiatement que l'appareil auxiliaire lui-même peut introduire des erreurs de même ordre ou des inexactitudes encore plus grandes que celles que l'on cherche à évaluer."

This latter assertion is directly at variance with the opinion which I had expressed on the same point in § 1 of my old paper (*Astron. Nachr.* vol. lvii. p. 260), in case the construction is adapted to its purpose (*bei zweckmässiger Einrichtung*), so that there must be some misunderstanding or some flaw of reasoning on the one side or the other. Where was the flaw? On the one hand, I gave credit to M. Loewy that an astronomer in his position would not make assertions without due consideration, so that my not perceiving, even after searching, what M. Loewy sees "immédiatement" might be due to some fault on my part. On the other hand, the very word "immédiatement" suggested a doubt, and this doubt was not lessened when, reading on, I found that his new method, so far as it refers to the flexure of the telescope, was merely one of the variations of my own proposal, which I had duly considered, and duly rejected, some years before the publication of my old paper. However, as M. Loewy's objections were for me rather enigmatical, I had to wait till further information might throw some light upon them; and I had to content myself meanwhile with occasional (unsuccessful) enquiries of astronomers, who were going, or had been, to the Paris Observatory, whether they could enlighten me, what meaning was to be attached to the words "effet de torsion que produit sur l'appareil la flexure du cube central," and how M. Loewy's own apparatus was fastened. In November last (1881) I got an opportunity of seeing the memoir of MM. Loewy et Périgaud, "Etude des flexions du grand cercle méridien . . . et de la forme des tourillons à l'aide de l'appareil imaginé par M. Loewy," extracted from vol. xvi. of the *Annales de l'Observatoire de Paris*; and when the subject of flexure came up incidentally at the December meeting, I made the statements referring to my old proposal and its first embodiment in Messrs. Cooke's apparatus, to M. Loewy's objections and to his description of his own apparatus, &c., which are alluded to on page 345. I expressed also my hope that, when Messrs. Cooke's apparatus should be exhibited at one of the

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meetings, those who took an interest in the matter would exercise their critical faculties upon it and compare it with M. Loewy's, described in his memoir.

M. Loewy has not waited for a little information about the actual instrument, but has, in his "Réponse," &c., already made a comparison, so that I have at last become enlightened about what his objections really are.

Strange to say, of all the "nombreux mouvements indépendants les uns des autres," which, according to M. Loewy, the lenses and the mirror of the apparatus undergo during the rotation of the instrument, he singles out the one: "que le flint et le crown peuvent se déplacer l'un par rapport à l'autre. . . ." Of course, if we assume great carelessness and negligence on the part of the observer as well as of the instrument-maker, that may be possible. But are there readers who object seriously to the employment of achromatic object-glasses for that reason? On the last page of his memoir, M. Loewy gives some account of an experiment (to which I shall have to refer further on) to ascertain whether the two lenses of the object-glass of the Bischoffsheim circle glide upon one another during the rotation of the telescope, and he draws the conclusion, "les deux disques constituent donc un bloc invariable," so that, according to his own showing, the fault in question is avoidable, even in the case of an object-glass of considerable aperture. But in the case of small object-glasses, such as those employed in my apparatus, the fault is not only far more easily guarded against, but, if it were needed, there is available a simple remedy—the cementing of the lenses. In reference to the mounting of his own lens, M. Loewy says (on page 6 of the "Etude," &c.): "Pour rendre absolument solidaires le verre et sa monture et éliminer tout mouvement relatif de ces deux appareils, on les a collés ensemble à l'arcanson." *Absolument* solidaires! If the panacea is to be considered so very efficacious in the one case, why should it lose its efficacy in the other? What becomes then of this, the only one of M. Loewy's objections which is not quite vague? But, it so happens, that in my apparatus it would not be really necessary that the two lenses, which form each of the two small object-glasses, should be "absolument solidaires." For they might be allowed to glide, and, provided the amount of the gliding is a regular periodical function of the direction of the telescope, its effect upon the results would be got rid of, in case the conditions laid down in § 3 of my old paper are complied with. Does it require more than a little reflection to see this?

In order to appreciate at their true value M. Loewy's other objections, let readers first examine the reasons which induced me some twenty years ago to reject that variation of my proposal, which M. Loewy treats as if it were a new method.

Astronomers who take an interest in the flexure problem, and who have ever read the "Description of the Transit Circle" in the Appendix of the *Greenwich Observations* of 1852 (reprinted in

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the volume of 1867) are acquainted with the experiments, which were made at Greenwich in September 1850, in order "to determine the absolute flexure of the telescope tube as attached to the axis." The Astronomer Royal's account of these experiments is found on page 17 of the description, and is as follows:—

"Before mounting the 12-ft. object-glass, an object-glass of about 3-ft. focal length was mounted in the centre of the cube, and a cross of wires was fixed at the object end, so that its image was formed upon the wires of the eye-piece. A better image would have been formed if two 6-ft. object-glasses had been used with their external surfaces opposed to each other. The image, however, was quite good enough for this determination. The place of the image at the eye end was measured with the declination micrometer when the telescope was horizontal with the object end north, and again when the object end was south. The difference of the two readings found on 1850 September 2 and September 14 (the two results being in precise accordance), was $0^{r}.204 = 0.00414$ in., one half of which, or 0·00207 in., was the effect of flexure in lowering one end with respect to the line passing through the centre and the other end. If the two ends yielded equally, the drop of each was sensibly $\frac{1}{1000}$ part of an inch.

"This determination, it will be remarked, is one of curiosity only."

So far the statement in the Greenwich volume. Years ago I have made some inquiries about the manner in which the cross of wires was really fixed, but have not succeeded in getting the desired information, so that I am not sure whether the inference which has been drawn from these experiments may not require some modification. However that may be, these experiments indicated a way to determine the *sum* of the flexures of the two ends of a telescope, and though this determination was avowedly one of curiosity only, and of no direct astronomical interest, it set me thinking whether the determination of the *difference* of the flexures of the ends of a telescope, and therewith that of the astronomical flexure, might not be accomplished by a suitable arrangement. When I had settled in my mind that this could be done by substituting for the cross of wires a mark or marks near the optical centre of the object-glass and by arranging the apparatus so that it should show in the plane of the webs or lines at the eye end not only an image of the mark, but also a reflected image of the webs themselves, and when I had thus found the simple theoretical solution of the problem, I had to consider and compare the advantages and disadvantages of various schemes for making this solution available in practice, and to select that which promised to be the most efficient.

In order that the apparatus may truly answer its purpose, it must be fastened to the instrument mechanically in such a way that its rotation is exactly determined by that of the divided circle, and it must be so constructed that the results can be

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freed from all systematic errors. The determination or elimination of any periodical errors represented by $\alpha \sin z + \beta \cos z$, and by similar terms depending on $3z$, $5z$, &c., can be directly effected by making the apparatus reversible, and this advantage can be secured in case the object-glass and the eye-end webs of the instrument are made equidistant from the axis of rotation. The optical part of the apparatus must, of course, be so arranged as to give sharp images of the mark and of the webs simultaneously. Which scheme promised then to be practically the best?

Taking first the case of a single object-glass, as in the Greenwich experiment, or the still simpler one of a single lens, the reflected image of the webs could obviously be procured by converting a portion of the surface of the lens into a concave mirror with a radius of curvature equal to the distance from the webs; but the question was whether the central or the outer part of the surface should be made the mirror. If the central portion was used for the purpose, the lens could be made symmetrical and reversible, as there would probably be no practical difficulty in converting the middle part of a double convex lens into a double concave one. But what sort of image of the mark would be got in that way? If the outer portion of the lens was formed into a mirror, there would be the practical difficulty of making the central portion of a double concave lens double convex. Of course, by abandoning the condition of reversibility and symmetry, the lens might be made a meniscus. But, in that case, how could the error of rotation of the mirror's axis be determined or eliminated? And how could sharp focusing be effected simultaneously for both images? The condition of reversibility may be maintained by leaving the middle part of the double concave mirror perforated, and by inserting in the perforation a double convex lens of proper focal length. By making the mirror lens thick enough the great advantage would be gained that the sides opposite to the pivots could themselves be converted into such plane mirrors as I require for the determination of the flexure of the axis and of the pivot errors. In order to get a better image and to allow exact focusing, two crossed lenses or two small achromatic object-glasses may then be inserted, instead of the single lens, in the perforation. The whole apparatus might indeed be made of glass, so that the mirrors and the end-pieces, which are fastened to the instrument, are formed out of one body. But though the making of such a body of glass might not be difficult, it could probably only be procured by special favour of the glassmaker, and the converting of it into the intended apparatus would also require special willingness on the part of the optician, so that the conditions of its construction would be rather exceptional.

Refraining then from assuming more than the ordinary resources of the optician's workshop, I had to consider the case in which the optical part of the flexure apparatus is composed of two lenses or two achromatic object-glasses and of a plane mirror,

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covering about one-half of their apertures, between them. Of the motions of rotation and translation to which the various parts of the apparatus are liable, two, and only two can affect the determination of the astronomical flexure, namely the error of rotation of the mirror, and the difference of translation of the optical centres of the two lenses or of the two object-glasses. Let these optical centres, p and q, the mark o on the great object-glass, and the assumed zero-point r of the eye-end micrometer be referred to a system of rectangular co-ordinates, one axis of which is the actual axis of rotation, while the other two axes rotate with the circle, the x axis being approximately in the direction of the axis of the telescope. In order to define the position of the mirror, let ρ denote that point in the plane of the micrometer webs which is normal to the plane of the mirror at the point where the x axis cuts it. As the effects of flexure in the two directions of the circle- and of the transit-observations are investigated separately, though in the same way, the ordinates of the five points $o\ p\ q\ r\ \rho$ in the two directions may be conveniently denoted by the same letters and as the points themselves, so that in the one case $o\ p\ q\ r\ \rho$ signify the distances of the five points from the plane passing through the x axis and the axis of rotation, and in the other case their distances from the plane passing through the x axis perpendicular to the axis of rotation. The micrometer readings O and R (in the notation of my old paper) at which the moveable webs coincide with the image of the mark and with their own reflected image represent the ordinates of these two points in reference to the zero-point of the micrometer. Their ordinates in the assumed system are therefore $r-O$ and $r-R$, in case the micrometer measures are reckoned positive in the opposite direction to that of the ordinates. As the line from the mark o to the optical centre p is parallel to the line from q to the image of the mark, we have, if the two lines are of unequal length in the proportion of $\kappa : 1$,

$$\frac{o-p}{\kappa} = q-(r-O) \quad \text{or} \quad o+\kappa r-p-\kappa q=\kappa O.$$

Further

$$r-R=\rho+q \qquad \text{or} \qquad r-\rho-q=R.$$

Hence the equation for the correction $o-r$ of the astronomical flexure

$$o-r+(1+\kappa)\rho-p+q=\kappa O-(1+\kappa)R,$$

which shows that, in addition to the error ρ of the rotation of the mirror, the difference $p-q$ in the motion of the two optical centres may affect the determination of flexure, and that it must be freed from the influence of these two sources of error. By making the apparatus reversible in reference to the axis of the telescope and accordingly symmetrical and equidistant from the mark

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and from its image, the equations become, since in that case $\kappa = 1$,

$$o + r - p - q = 0$$
$$r - \rho - q = R$$

and therefore, if the flexure correction $o - r$, as a function of the zenith-distance or of the circle-reading z, is now called fz,

$$fz + 2\rho - p + q = O - 2R.$$

Assuming that o and $p - q$ are likewise functions of z, their periodical terms of the form $\alpha \sin z + \beta \cos z$, and similar terms depending on the uneven multiples of z, have opposite signs in the reversed position of the apparatus, the mean of the observations in the two positions becomes therefore free from these terms, and the remaining question is merely whether the apparatus is constructed and mounted with sufficient care so as to prevent the existence of other terms and terms which are not functions of z (as, for instance, the shaking of the mirror and lenses).

If the optical part of the apparatus could be constructed effectively out of one piece of glass, there would be only one optical centre instead of two, the ordinates p and q would be identical, and therefore

$$o + \kappa r - (1 + \kappa)\, q = \kappa O, \text{ and}$$
$$fz + (1 + \kappa)\, \rho = \kappa O - (1 + \kappa) R,$$

the equation for R being the same as before. Provided that the O and R observations are made at the same time or while the telescope is undisturbed, so that the value of q remains the same for both, the flexure equation would then be affected only by the error ρ of the rotation of the mirror's axis. But is there any trustworthy method available for determining this error without making the apparatus reversible? Or, in making it reversible, is there any way of overcoming the optical difficulties which I have mentioned before?

It would lead me too far here to speak of other schemes for the practical application of the principle of my proposal, as the question at issue is narrowed to a comparison between the scheme which I adopted and the meniscus scheme. In considering the latter, I could not think of a trustworthy method for testing and determining the all-important error of rotation of the axis, nor get over the optical difficulty. In the other case I could plan the apparatus so as to provide for the determination or elimination of any plausible error which might affect the results, to secure, even under variable conditions, good images by arranging for exact focusing without disturbing the mirror, and to test and control the efficiency of the apparatus in several ways. Hence I had no difficulty and no hesitation in coming to a decision between the two schemes.

I will now turn again to M. Loewy's assertions and objections.

When, in reading his paper in the *Comptes Rendus* of Dec. 9, 1878, I found that what M. Loewy represented as a new method

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was merely one of my old rejected acquaintances, I was, of course, curious how he managed to meet the difficulties on account of which I had discarded the meniscus scheme; but I had to wait till I saw the *Étude* &c., before I could learn what provisions he makes for getting good images and for exact focusing, and in what manner he solves the problem of determining the error of rotation of the mirror.

As regards the images, the mirror is first shifted in the direction of the optical axis till a satisfactory reflected image of the webs is seen. "La place du miroir étant ainsi obtenue, s'il arrive que les traits de l'objectif ne donnent pas dans le champ une image nette, on corrige l'une des deux surfaces [of the convex-concave lens] jusqu'à ce que la netteté soit obtenue" (v. *Etude*, page C. 7). Some information about the image of the mark may be gathered from the *Nota* on the last page (C. 120). "Pour vérifier si les deux verres de l'objectif ne glissaient pas l'un sur l'autre . . ., nous avons au milieu du disque intérieur fixé une petite goutte d'argenture . . ., sur laquelle nous avons tracé deux traits en croix, et nous avons mesuré . . . les distances de cette croisée de fils et d'un point de la surface extérieure de l'objectif, c'est-à-dire de la croisée de deux traits; la différence a éte constante . . . Les deux disques constituent donc un bloc invariable." The two crosses are separated by the thickness of the great object-glass, so that the conjugate foci, where their images are formed, are separated by a corresponding interval. But, of course, if the two images can be seen simultaneously with such distinctness as to allow exact measurements, why should M. Loewy trouble himself about exact focusing, or make provision for it? Though I have not had an opportunity of testing the optical part of Messrs. Cooke's apparatus, I need hardly say that their two object-glasses will certainly be found unfit to allow such a feat as M. Loewy's to be performed. In case the marks on the two lenses of the great object-glass are not to be put on their inner surfaces (and the reason to be taken into consideration is, that in that case the marks could not well be formed by cross-lines, but would have to be points or little circles), but on their outer surfaces, the contrivance for exact focusing must be resorted to, and the marks examined separately. It is to be noted that the diameter of the effective aperture of the convex-concave lens, which M. Loewy employs, varies (C. 3) for different telescopes from 8 to 15^{mm}, or is at most $0^{in.}6$.

And now, in what way does M. Loewy solve the problem of determining the error of rotation of the mirror's axis, the unknown ρ in the equation $fz+(1+\kappa)\rho=\kappa O-(1+\kappa)R$, on the elimination of which the determination of the flexure correction fz depends? On page C. 9 he states: "l'axe de suspension de notre appareil étant justement perpendiculaire à [l'axe est-ouest], ce mouvement angulaire [autour de l'axe est-ouest] se trouve rendu impossible par l'effet même de la construction. Du reste, la comparaison des résultats trouvés pour l'objectif et le réticule

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a justifié cette assertion." That M. Loewy has satisfied himself on this point may be learnt from page C. 28, where he says: "après avoir démontré rigoureusement, par la concordance des valeurs de a [a denoting the motion of translation of the optical centre or the q of the equations in the present note], qu'il n'existe pas dans notre appareil de mouvement tournant autour de l'axe [est-ouest], ce qui était du reste à prévoir, comme nous l'avons dit, nous avons, pour plus de rigueur, repris l'ensemble des équations . . ., pour en tirer . . . les trois inconnues f_r, f_o and a" [the r, o and q of the equations before given].

It appears, therefore, from M. Loewy's assertions, first, that the error ρ, or the rotatory motion in question, is rendered *impossible* by the very construction of the apparatus; and, secondly, that he has in some way *rigorously demonstrated* that in his apparatus it does not exist.

For those readers who have not access to the *Étude* it may be necessary at least to indicate the manner of M. Loewy's demonstration. He first simplifies the equation for the observations of the reflected image $r-\rho-q=R$, by simply omitting the unknown ρ altogether, so that the equation becomes $r-q=R$, or, in his notation,

$$f_r - a = m.$$

The equation for the image of the mark is

$$f_r - a\left(1+\frac{\alpha}{\beta}\right)+f_o\frac{\alpha}{\beta}=n$$

[$\frac{\beta}{\alpha}$ and n being the κ and O of the former equations]. If the m and n observations are made at the same time, or while the telescope is left undisturbed, so that the a in the two equations have strictly the same value, the flexure correction f_o-f_r is quite unaffected by whatever the value of a may be, for, as the unknown ρ has been got rid of, the flexure correction becomes simply

$$f_o - f_r = \frac{\beta}{\alpha}n - \left(1+\frac{\beta}{\alpha}\right)m.$$

The dates of the m and n observations show that the observations have not been made at the same time, and consequently that the simple precaution to eliminate a entirely has not been taken; moreover, M. Loewy requires the value of a for his attempt to find the flexure of the axis. Otherwise, the knowledge of a would be a matter of indifference, as it is in the case of my apparatus, where neither the investigation of the flexure of the telescope nor that of the axis is affected by it. In order to determine the value of a, M. Loewy repeats the observations, after having attached extra weights outside the framework which holds the lens, and after having thus altered the whole weight of the apparatus. He then makes the hypothesis ("évi-

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dente *à priori*," as he says, p. C. 17) that the value of a is in proportion to the whole weight, satisfies himself in the case of the great meridian circle that "on peut donc conclure l'absolue proportionalité des effets dus à la pesanteur de l'appareil" (C. 24), and in the case of the Bischoffsheim circle he simply assumes the correctness of the hypothesis. From the accordance of the values of a deduced from the m and from the n observations he concludes the non-existence of any rotatory motion, and he calls this conclusion a rigorous demonstration. But what hypothesis he makes in coming to this conclusion M. Loewy does not say. Though in the investigation of the side-flexure the same hypothesis is disproved by the observations (C. 62), M. Loewy is not disconcerted. "Cette particularité fâcheuse provient de ce que l'axe de notre appareil n'était pas fixé d'une manière assez solide dans les coussinets;" and he thinks l'arcanson would have prevented it.

After my first reading of the *Étude*, I had wondered that the apparatus should have been used in one position only. For, though it cannot be reversed in reference to the axis of the telescope, it can at least be inverted or reversed in reference to the axis of rotation. And though agreement of the results obtained from the observations in the two positions might not prove much, and could not be a test such as agreement after reversion in reference to the axis of the telescope would be, it would at least be worth something. When I made my remarks at the December meeting 1881, I little anticipated what curious revelations they would bring to light. From M. Loewy's *Réponse*, &c., on page 288, readers have now learnt that, after all, observations in the inverted position of the apparatus have been made, and that he is in possession of evidence of very decisive character. I must beg my readers to refer to the paper itself so that I need not quote the whole of the context. M. Loewy states there: "nous avons retourné l'appareil pour opérer dans des conditions différentes, et . . . nous avons constaté que le mouvement de l'instrument dans la position inverse était tout-à-fait différent du premier et qu'il était impossible d'établir aucune relation entre les deux." The conclusion which M. Loewy draws is found a dozen lines further down: "Il est donc évident que ce qui se présente pour un appareil aussi simple que le mien existe *à fortiori* pour la disposition beaucoup plus compliquée de M. Marth."

But what are the true conclusions which are to be drawn from his statements? If it has been found impossible to establish any relationship between the motions of the instrument in the two positions, then it is evident that there is something wrong about the apparatus. If M. Loewy's statement implies that this want of any relationship is established for the astronomical flexure, the source of the mischief is obvious, for in the meniscus scheme the only cause which can produce such an effect is the rotatory motion ρ. After M. Loewy's previous assertions

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in the *Étude*, *first*, that the existence of this rotatory motion is rendered impossible by the construction of his apparatus, and, *secondly*, that he has rigorously demonstrated that it does not exist, his statements on page 288 lead, on the contrary, to the logical conclusion, *thirdly*, that this rotatory motion *does* exist in his apparatus, that hitherto the law which it follows has not been found, and that consequently the results obtained cannot be cleared from the influence of this rotatory motion.

The inference which M. Loewy himself draws is, of course, quite different. Unconscious of what the fact that his apparatus does not sustain even the test of mere inversion really proves, he deduces from it and from some other peculiarity of his apparatus the curious "Il est donc évident," &c., which I have quoted. Will readers consider a little the construction of the two instruments, the one carefully planned and constructed so as to form a compact body which can be inserted in its place and taken out and re-inserted in its inverted and reversed position with the greatest ease and confidence; the other constructed with such want of ordinary forethought and caution that it cannot be put in its place or taken out without being taken to pieces—and will they then form their own opinion?

M. Loewy's repeated assertions of the simplicity of his apparatus receive some rather curious and telling commentaries in the Paris Reports. In the *Rapport Annuel sur l'état de l'Observatoire de Paris pour l'année* 1880, on p. 3, Admiral Mouchez states: "M. Loewy s'occupe activement de l'étude du cercle du jardin . . .; pendant quatre mois, cet instrument a été distrait du service régulier pour l'étude de la flexion." And in the *Rapport pour* 1881, p. 4: "En tenant compte que, pendant cinq ou six mois, une partie du personnel a été employée jour et nuit à la délicate opération de la mesure de la flexion et des erreurs de division du cercle du jardin, et que, pendant cette même période, les observations astronomiques ont été nécessairement suspendues à cet instrument, on jugera que le personnel du service méridien a . . . rempli sa tâche."

I have probably given sufficient explanations to enable anyone to estimate the statements and assertions and objections in M. Loewy's *Réponse* one by one at their true value, and to judge for himself. M. Loewy sums up his objections in the following sentence: "En résumé, pour étudier la variation d'une ligne de visée unique, pendant la rotation de la lunette, M. Marth admet, contrairement à la réalité, l'invariabilité des cinq lignes de visées différentes provenant des cinq pièces optiques composant son instrument."

The phrase sounds very well, but is there any truth in M. Loewy's assertion? I assume the five pieces of glass to be in motion, and examine the effect of their motions according to the optical theory which I hold to be correct (v. p. 346), and according to which I have to do with the investigation of the motions of two points and of a plane. Since M. Loewy asserts

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that I admit the invariability of five lines of vision, he must have some optical theory according to which there are five such lines "provenant des cinq pièces optiques" to be considered. What five lines of vision are meant? By what points are they defined, or through which points do they pass?

When I get enlightened about these five lines, there will perhaps be no difficulty in pointing out where the flaw in the reasoning on the one side or the other is to be found, and I will state the result of my examination in a Postscript. The settlement of this question will also settle whether what M. Loewy in his statement quoted at the beginning of this paper asserts is true or not.

For the present I will conclude with inviting my readers to make a little comparison of M. Loewy's method of determining the pivot-errors, not with my own method, but with the old well-known method of examining the pivot-errors by measuring the coordinates of dots at the ends of the pivots by means of microscope-micrometers fixedly mounted upon the piers. The method was first practically tried, if I am not mistaken, by Taylor at Madras half a century ago, though with inadequate means, and it has since been employed at Greenwich, at Cambridge, at Paris, and elsewhere, and, so far as I know, it has given satisfaction.

In his own method M. Loewy likewise determines the coordinates of a dot near the axis of the perforated pivot in reference to a fixed point; but there is this difference, that, while in the old method the fixed point is the zero point of the micrometer itself and the coordinates of the dot are directly referred to it, in M. Loewy's arrangement the fixed point, or what is intended to be the fixed point, is produced by a beam of light throwing a luminous image of the centre of a perforated disk upon the glass plate containing the dot, and the relative positions of the dot and of the luminous image are then measured by the eye-end micrometers, as their images are visible there by means of a part of the flexure-apparatus acting as a prism.

And then, M. Loewy's actual application of the altered method? For some reason, which is not explained, the investigation of the pivot-errors as well as that of the flexure-errors is confined to a number of directions of the telescope extending over only one-half of the rotation, and is not carried out for the other half. The results are consequently incomplete and deprived of that degree of trustworthiness which would have been attainable had the measurements been made in the same number of directions but distributed over the whole circumference. The investigation of the errors of the western pivot of the great meridian circle leads merely to the conclusion "... nous sommes en droit d'admettre que nous pouvons, sans qu'il en résulte d'erreurs sensibles pour les réductions, considérer le tourillon percé comme cylindrique ..." M. Loewy then proceeds (*Étude*, p. C. 93): "Il eût été, sans nul doute, très intéressant de

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faire la même étude de la forme du tourillon *est*; mais, ce tourillon n'étant pas percé, il ne nous était pas possible d'appliquer nos procédés.

"Toutefois des expériences préalables, exécutées directement sur chaque tourillon avec un comparateur à niveau très sensible, ont confirmé nos conclusions pour le tourillon *ouest* et donné des résultats très satisfaisants pour la cylindricité du tourillon *est*.

"De plus, l'inclinaison mesurée avec le niveau restant la même dans toutes les directions de la lunette, on peut conclure que ce tourillon *est* se trouve également cylindrique."

This, then, is the upshot of the investigation which fills pp. C. 78 to 93 of the *Étude*! In the case of the Bischoffsheim circle such an investigation has not yet been made, the reason being given on p. C. 120:

"Ici, comme pour le grand méridien, devait se placer l'étude de la forme du tourillon percé. Mais . . . il s'est produit, dans la taille de la lentille tournée vers le tourillon, un petit effet de cylindricité qui ne nous a pas permis de voir simultanément les fils verticaux et horizontaux et, par suite, de procéder à la recherche de la forme de ce tourillon. Il eût fallu, pour l'entreprendre, préalablement retoucher la partie latérale du disque de verre. Or, le beau temps étant survenu, nous n'avons pas voulu suspendre plus longtemps les travaux d'observations, et nous avons remis cette étude complémentaire à une époque ultérieure plus convenable."

From the "*du* tourillon percé" it seems that also in the case of the new Bischoffsheim instrument only one of the two pivots is perforated, so that, at best, M. Loewy's variation of the old method could not be applied except to that pivot.

But is there any well-founded reason against the application of the old method itself? Perhaps even the microscope-micrometers which were used by M. Villarceau in his "Étude du Mouvement de Rotation de la Lunette Méridienne," published 1863, in vol. vii. p. 307 ff. of the *Annales de l'Observatoire de Paris*, may be still available, so that it would only be required to mount and fix them properly, and to have the dots made, in order to be ready for the examination of both pivots. Or is there anything in which the old method is not superior to M. Loewy's variation? In simplicity, in trustworthiness, in applicability—is the comparison not all in its favour?

But then, the old method is totally ignored in what M. Loewy says on p. C. 2 of the *Étude*:

"A l'aide du nouvel appareil, il nous a encore été permis de résoudre un problème aussi intéressant au point de vue de la Mécanique qu'au point de vue de l'Astronomie. Il s'agissait de rechercher si la forme des tourillons est rigoureusement cylindrique. La seule méthode connue jusqu'ici pour résoudre cette question est d'une application difficile: elle exige l'installation de piliers et de collimateurs spéciaux, aussi n'a-t-elle été essayée qu'exceptionellement dans un observatoire ou deux."

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How is this ignoring of a well known method to be accounted for?

When, in the spring of 1880, Prof. Houzeau was in London, and I had the pleasure of meeting him at a friend's, he suggested, as a method for the determination of the flexure of a telescope the fixing of a plane mirrer in front of the middle of the object-glass and the observation of the reflected image of the webs in different positions of the telescope. It was pointed out in reply that that would not do for determining the *astronomical* flexure, for he would find, on examination, that in that way he would far more likely get the sum of the flexures of the two halves of the telescope than their difference, which he wanted to get. I remembered the unfavourable circumstances which had prevented proper discussion, when I read in the *English Mechanic* of Feb. 24, 1882, vol xxxiv. p. 587, an article, "Measuring the Flexure of Telescopes," from which it appeared that Prof. Houzeau had brought his suggestion before the Belgian Academy of Sciences. I find that Houzeau's paper, "Sur un moyen de mesurer la flexion des lunettes," was brought before the Academy on Oct. 8, 1881, and that from the article in the *English Mechanic* readers may learn all that is required.

What is practically the same suggestion has since been published by Mr. Schaeberle in the *American Journal of Science* for May 1882, p. 374.

In my method of finding the astronomical flexure, the flexure of the eye-end half of the telescope tube is determined by observations of the image of the webs reflected from a plane mirror before an object-glass. Why not use the same means to determine the flexure of the whole tube? The thought is so simple and, for a few moments, so tempting. The equations which show what the micrometer readings R represent are, in the case of the

$$\text{half tube, } \nu-\rho-q=R,$$

and in that of the

$$\text{whole tube, } \nu-\rho-o=R,$$

and therefore apparently analogous. But there is this great difference, that in the first case ρ can be determined, and in the second case it cannot and remains mixed up with the astronomical flexure. What is the value of ρ likely to be? If the axis of the object-glass retains the same relation to the axis of the object-end half of the telescope tube, it will always pass through a certain point near the centre of the instrument. If it does so, the value of ρ will be $=-2o$, and the equation for R becomes $r+o=R$, or the observation of the reflected image gives the sum and not the difference of the flexures of the two ends of the telescope. But, of course, this is merely a conjecture, though perhaps a plausible conjecture. The astronomical flexure is not affected by any rotatory motion of the object-glass round its

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optical centre, but merely by the motion of translation of this centre, while the observations of the image of the webs reflected from a mirror attached to the front of the object-glass are affected by any wabbling of the latter, and may therefore show many variations, which will not be of any importance for the astronomical observations.

At the time of my conversation with Prof. Houzeau I was not aware that the suggestion had ever been carried out in practice, and it is only in hunting up in the *Comptes Rendus* some of Porro's papers that I have since come upon the track of Secchi's experiments. Prof. Houzeau says in his paper: "Heurensement il existe un moyen . . . auquel on s'étonne que l'on n'ait pas songé auparavant." But it turns out that Secchi not only thought of it long ago, but practically tried it. His paper, "*Sulla maniera di determinare la flessione dei tubi nei grandi strumenti astronomici*," which I know only by its title (to be found also on p. 900 of the *Bibliographie de l'Astronomie*, par Houzeau et Lancaster), has been published in Tortolini's *Annali*, v., 1854, and in the *Corrispondenza Scientifica in Roma*, iii., 1855. In vol. xxxviii. p. 735 of the *Comptes Rendus* Porro refers to an account of Secchi's paper in Moigno's publication *Cosmos* of 1854. As neither Secchi's paper nor the fourth volume of *Cosmos* are in the library of the Royal Astronomical Society, and as probably not a few readers will like to learn something about Secchi's experiments, it seems worth while to reprint the essential part of Moigno's statement. I, for one, should have been glad to have been acquainted with it long ago.

"Il [Secchi] a fixé à la partie de sa lunette qui portait l'objectif, et au devant de l'objectif lui-même, un petit miroir plan qui venait d'un sextant de Munich, et il a éclairé obliquement les fils du réticule oculaire . . . En procédant de cette manière, le P. Secchi s'est aperçu que les flexions étaient loin de suivre la loi des sinus des distances zénithales. Il y eut même de telles anomalies dans les résultats obtenus des deux côtés du zénith, les observations au nord étaient tellement en désaccord avec les observations du côté sud, que le P. Secchi resta quelque temps dans la plus grande perplexité sur l'origine de ces divergences.

"Enfin il s'aperçut qu'il suffisait de laisser la lunette quelque temps en repos dans une position non verticale, dans la position horizontale, par exemple, pour que sa flexion, nulle d'abord, dépassât peu à peu un angle de deux secondes. La lenteur avec laquelle s'accomplissait la flexion, et le temps qu'elle exigeait pour disparaître étaient les causes véritables de toutes les anomalies constatées. Mais comment tenir compte dans les observations d'éléments si peu susceptibles de mesures? L'habile astronome propose d'adapter pour cela à toutes les lunettes les appareils propres à éliminer l'erreur de collimation, à se mettre à l'abri de la flexion des tuyaux télescopiques. Ces appareils pourront consister, soit dans les objectifs de M. Porro, soit dans

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le petit miroir plan dont nous venons de parler, soit dans un miroir concave fixé contre l'objectif, soit enfin dans une lentille intermédiaire qui se placerait de façon à donner sur le réseau oculaire l'image d'un réticule appliqué contre la lentille objective. Cette dernière méthode serait préférable au dire du savant astronome, car elle permettrait d'éliminer les flexions et pendant le jour et pendant la nuit, tandis que le peu de lumière réfléchie par les fils ne permet guère d'observer leur image renvoyée par le miroir quand les rayons du jour pénètrent librement dans la lunette."

Porro's object-glass, spoken of by Secchi, is, so far as I know, a merely theoretical one in which the radius of curvature of the fourth surface is made equal to the focal length, so that the image of the webs may be reflected from this surface itself (vide *Comptes Rendus*, vol. xxxvii. p. 752). In his note (vol. xxxviii. p. 735), already quoted, Porro says of the result of Secchi's mode of measuring, ". . . la mesure ainsi prise du déplacement de l'image du fil ne représentera pas la flexion, ni même une fonction mathématiquement assignable de cette quantité . . ." Secchi, I presume, has acquiesced in the justice of Porro's remark, as upon consideration, no doubt, Prof. Houzeau and Mr. Schaeberle will do.

But though measurements of the reflected image procured by means of a mirror attached to the object-glass cannot lead to the determination of the astronomical flexure of a telescope, they will furnish evidence of the real motions of the axis of the object-glass, the study of which will be of interest to the astronomer as well as to the mechanic, and it seems to me, therefore, that in the case of very large object-glasses it would be worth while to make such measurements.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLIV. FEBRUARY 8, 1884. No. 4.

E. J. STONE, M.A., F.R.S., President, in the Chair.

Thomas Allsop, Oakdene, Bexley, Kent;
Wentworth Bennett, Cadbury, Castle Cary, Somerset;
William Coleman, The Shrubbery, Buckland, Dover;
Joseph Davis, 6 Kennington Park Road, S.E.;
Thomas Read, B.A., 10 Barnpark Terrace, Teignmouth; and
Frederick William Watkin, B.A., St. Paul's School, E.C.;

were balloted for and duly elected Fellows of the Society.

REPORT OF THE COUNCIL TO THE SIXTY-FOURTH ANNUAL GENERAL MEETING OF THE SOCIETY.

The following table shows the progress and present state of the Society:—

	Compounders	Annual Subscribers	Non-resident	Mathematical Society	Total Fellows	Associates	Patron	Grand Total
December 31, 1882 ...	230	361	2	5	598	46	1	645
Since elected	+2	+28	...	...	...	+4	...	...
Deceased	−6	−8	...	−2	...	−1	...	...
Resigned	...	−9	...	...	...	...	...	...
Expelled	...	−1	...	...	...	...	...	...
December 31, 1883 ...	226	371	2	3	602	49	1	652

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Mr. Barrow's Account as Treasurer of the Royal

RECEIPTS.

	£	s.	d.	£	s.	d.
Balances at Bankers, Jan. 1, 1883:						
Credited in pass book	514	4	9			
Amounts not credited	16	0	6			
On deposit account at Bankers'	500	0	0			
				1030	5	3
Balance in hand of Assistant Secretary on account of Turnor Fund, Jan. 1, 1883	6	15	4			
,, in hand of Hon. Secretary on account of payments for Computation of Ephemerides, Jan. 1, 1883	12	10	0			
				19	5	4
*Dividend on £7,500 Consols	108	15	0			
* ,, £5,700 New 3 per cent. Stock ...	83	3	9			
* ,, £7,500 Consols	110	3	2			
* ,, £5,700 New 3 per cent. Stock ...	83	14	5			
Dividends on £500 Metropolitan Stock	7	7	0			
Interest on money on deposit at Bankers' ...	10	6	7			
				403	9	11
Received on account of Subscriptions:						
Arrears	195	6	0			
261 Annual Contributions	548	2	0			
28 Admission Fees	58	16	0			
26 First Contributions	43	1	0			
				845	5	0
2 Composition Fees				42	0	0
Sales of Publications:						
At Williams & Norgate's, 1882	48	7	3			
At Society's Rooms, 1883	33	11	7			
				81	18	10
				£2,422	4	4

* Half-years' dividends.

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Astronomical Society, from Dec. 31, 1882, to Dec. 31, 1883.

EXPENDITURE.

	£	s.	d.	£	s.	d.
Assistant Secretary: Salary	225	0	0			
„ „ for assistance in editing Society's publications...	50	0	0			
				275	0	0
Income Tax and House Duty				11	3	1
Fire Insurance				7	16	6
Printing: Spottiswoode & Co.	345	1	0			
Engraving and Lithography	33	8	2			
Computation of Ephemerides in *Monthly Notices*, 1882	12	10	0			
Computation of Ephemerides in *Monthly Notices*, 1883	50	0	0			
				440	19	2
Turnor and Horrox Funds: Purchase of Books for Library	19	15	2			
Binding Books in Library	40	11	7			
Petty Library expenses	3	7	1			
				63	13	10
Decorations and Repairs to Society's Apartments: Burdett & Sons	491	8	9			
Mr. Ruddle: for preparing Specifications, &c. ...	15	15	0			
Examining and cleaning Heating Apparatus: Haden & Son...	5	6	0			
Carpet, Floorcloth, &c., and cleaning Carpets: Tyler & Co.	17	9	2			
				529	18	11
House Expenses	31	13	7			
Wages	22	4	0			
Postage, &c.	55	0	11			
Carriage of parcels, &c.	1	11	10			
Stationery and office expenses	9	0	8			
Expenses of meetings	20	0	0			
Coals and gas	65	10	11			
Sundry repairs, &c.	13	17	9			
Sundries	6	16	8			
				225	16	4
Lee fund				55	0	0
Mrs. Jackson-Gwilt's annuity				8	19	0
Purchase of £500 Metropolitan Board of Works stock at 98, including commission				490	12	6
Bankers' commissions on cheques			3	0		
Powers of attorney for receipt of dividends ...			11	6		
Cheque book			8	4		
				1	2	10
Balance at bankers' Dec. 31, 1883	291	9	6			
„ in hand of Assistant Secretary on account of Turnor and Horrox Funds...	7	0	2			
„ in hand of Assistant Secretary on Petty Cash account...	10	18	11			
				309	8	7
Due to Assistant Secretary on Petty Cash account, Jan. 1, 1883				2	13	7
				£2,422	4	4

Audited and found correct, Jan. 5, 1884.

G. L. Tupman,
Robt. J. Lecky.
C. G. Talmage.

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Assets and present property of the Society, January 1, 1884:—

	£	s.	d.	£	s.	d.
Balance at Bankers', Dec. 31, 1883	291	9	6			
„ in hand of Assistant Secretary on account of Turnor and Horrox Funds... ...	7	0	2			
on Petty Cash account	10	18	11			
				309	8	7
Due on account of subscriptions:						
1 Contribution of 6 years' standing ...	12	12	0			
1 „ 4 „ ...	8	8	0			
14 „ 3 „ ...	88	4	0			
27 „ 2 „ ...	113	8	0			
61 „ 1 „ ...	128	2	0			
3 Admission fees and first contributions ...	9	9	0			
				360	3	0
Due from Messrs. Williams & Norgate (for sales of Publications during 1883)				28	10	3

£7,500 Consols, including the Lee Fund, the Turnor Fund, and the Horrox Memorial Fund.

£5,700 New 3 per cent. Stock, including Mrs. Jackson-Gwilt's gift (£300).

£500 Metropolitan Board of Works Stock.

Astronomical and other MSS.; Books, Prints, Instruments, and Furniture.

Unsold Publications of the Society.

Two Gold Medals.

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Report of the Auditors.

We, being the duly appointed Auditors, beg to lay before this General Meeting of the Royal Astronomical Society the following Report:—

We have examined the Treasurer's account, and an account of the assets and property of the Society, and have found and certified the same to be correct.

The receipts and expenditure for the past year are as stated in the Treasurer's account. The cash in hand on December 31, 1883, including the balance at the bankers', amounted to 309*l.* 8*s.* 7*d.*

The sum of 500*l.* has been invested in Metropolitan Board of Works Stock: the amount of funded property standing in the name of the Society is therefore 13,700*l.*, being an increase of 500*l.* from the previous year. The sum of 500*l.* on deposit at the bankers' has been withdrawn to pay for the decorations and repairs to the Society's rooms.

The books, instruments, and other effects have been examined and found in a satisfactory condition, so far as their safe keeping is concerned.

We have laid on the table a list of the names of those Fellows who are in arrear for sums due at the last Annual General Meeting, with the amount due against each Fellow's name.

G. L. Tupman.
C. G. Talmage.
Robt. J. Lecky.

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Stock in hand of volumes of the *Memoirs*:—

Vol.	At Society's Rooms	At Williams & Norgate's	Vol.	At Society's Rooms	At Williams & Norgate's
I. Part 1	6	...	XXVIII.	388	...
I. Part 2	41	...	XXIX.	415	...
II. Part 1	52	...	XXX.	165	...
II. Part 2	19	...	XXXI.	147	...
III. Part 1	67	1	XXXII.	162	...
III. Part 2	86	1	XXXIII.	168	1
IV. Part 1	81	3	XXXIV.	168	6
IV. Part 2	92	3	XXXV.	111	6
V.	108	4	XXXVI.	197	12
VI.	127	3	XXXVII. Part 1	345	7
VII.	151	3	XXXVII. Part 2	294	6
VIII.	129	3	XXXVIII.	284	2
IX.	128	3	XXXIX. Part 1	257	4
X.	148	...	XXXIX. Part 2	262	4
XI.	155	...	XL.	288	2
XII.	160	...	XLI.	440	2
XIII.	165	1	XLII.	250	5
XIV.	373	3	XLIII.	264	1
XV.	141	...	XLIV.	250	1
XVI.	168	1	XLV.	301	3
XVII.	149	2	XLVI.	328	5
XVIII.	149	...	XLVII. Part 1	2	...
XIX.	155	...	XLVII. Part 2	20	...
XX.	156	...	XLVII. Part 3	1	...
XXI. Part 1	314	...	XLVII. Part 4	20	...
XXI. Part 2	99	...	XLVII. Part 5	21	...
XXI. 1 & 2 (together)	63	1	XLVII. Part 6	21	...
XXII.	157	...	XLVII.	301	9
XXIII.	152	1	Index to *Memoirs*	649	1
XXIV.	159	1			
XXV.	171	...			
XXVI.	174	1			
XXVII.	428	...			

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Stock in hand of volumes of the *Monthly Notices*:—

Vol.	At Society's Rooms	At Williams & Norgate's	Vol.	At Society's Rooms	At Williams & Norgate's
I.	72	...	XXIV.	23	...
II.	74	...	XXV.	7	...
III.	...	...	XXVI.	9	...
IV.	...	...	XXVII.	3	...
V.	...	...	XXVIII.	72	1
VI.	39	...	XXIX.	54	...
VII.	2	...	XXX.	67	2
VIII.	139	2	XXXI.	98	...
IX.	23	3	XXXII.	119	5
X.	174	1	XXXIII.	103	2
XI.	186	1	XXXIV.	83	2
XII.	11	2	XXXV.	66	1
XIII.	150	3	XXXVI.	33	1
XIV.	109	3	XXXVII.	40	3
XV.	128	2	XXXVIII.	103	2
XVI.	108	3	XXXIX.	103	1
XVII.	136	1	XL.	116	3
XVIII.	166	...	XLI.	123	3
XIX.	57	...	XLII.	129	4
XX.	34	...	XLIII.	130	12
XXI.	17	...	Index to *Monthly Notices*	584	1
XXII.	32	...			
XXIII.	28	...			

In addition to the above volumes of the *Monthly Notices*, the Society has a considerable stock of separate numbers of nearly all the volumes. With the exception, however, of Vols. XXXVI. to XLIII. no complete volumes can be formed from the separate numbers in stock.

Instruments belonging to the Society.

No. 1. The *Harrison* clock.
„ 2. The *Owen* portable circles, by Jones.
„ 3. The *Beaufoy* circle.
„ 4. The *Beaufoy* transit instrument.
„ 5. The *Herschel* 7-foot telescope.
„ 6. The *Greig* universal instrument, by Reichenbach and Ertel. The transit telescope, by Ultzschneider and Fraunhofer, of Munich.

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No. 7. The *Smeaton* equatoreal.
„ 8. The *Cavendish* apparatus.
„ 9. The 7-foot Gregorian telescope (late Mr. Shearman's).
„ 10. The variation transit instrument (late Mr. Shearman's).
„ 11. The universal quadrat, by Abraham Sharp.
„ 12. The *Fuller* theodolite.
„ 13. The standard scale, by Troughton and Simms.
„ 14. The *Beaufoy* clock, No. 1.
„ 15. The *Beaufoy* clock, No. 2.
„ 16. The *Wollaston* telescope.
„ 17. The *Lee* circle.
„ 18. The *Sharpe* reflecting circle.
„ 19. The *Brisbane* circle.
„ 20. The *Baker* universal equatoreal.
„ 21. The *Reade* transit.
„ 22. The *Matthew* equatoreal, by Cooke.
„ 23. The *Matthew* transit instrument.
„ 24. The *South* transit instrument.
„ 25. A sextant, by Bird (formerly belonging to Captain Cook).
„ 26. A globe showing the precession of the equinoxes.

The *Sheepshanks* collection :—

„ 27. (1) 30-inch transit instrument, by Simms, with level and two iron stands.
„ 28. (2) 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.
„ 29. (3) $4\frac{6}{10}$-inch achromatic telescope, about 5 feet 6 inches focal length; finder; rack motion; double-image micrometer; two other micrometers; object-glass micrometer; one terrestrial and ten astronomical eyepieces, applied by means of two adapters; equatoreal stand, and clock movement.
„ 30. (4) $3\frac{1}{4}$-inch achromatic telescope, with equatoreal stand; double-image micrometer; one terrestrial and three astronomical eyepieces.
„ 31. (5) $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.
„ 33. (7) 2-foot navy telescope.
„ 34. (8) Transit instrument of 45 inches focal length; with iron stand, and also Ys for fixing to stone piers; two axis levels.
„ 35. (9) Repeating theodolite, by Ertel, with folding tripod stand.
„ 36. (10) 8-inch pillar sextant, by Troughton, divided on platinum, with counterpoise stand and artificial horizon.

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No. 37. (11) Portable zenith telescope and stand, $2\frac{3}{4}$-inch aperture and 26 inches focal length; 10-inch horizontal circle and 8-inch vertical circle, read to 10″ by two verniers to each circle.

„ 38. (12) 18-inch Borda repeating circle, by Troughton, $2\frac{1}{8}$-inch aperture and 24 inches focal length; the circles divided on silver, the horizontal circle being read by four verniers, and the vertical circle by three verniers, each to 10″.

„ 39. (13) 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms; circle divided on silver, reading to 10″; a 5-inch circle at eye-end reading to single minutes; horizontal circle 9 inches diameter in brass, reading to single minutes.

„ 40. (14) A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, reading to 10″; two sets of adjusting plates; tripod stand with enclosed telescope; heavy stand for theodolite; Y piece of level; two large and three small ground-glass bubbles divided; level collimator, object-glass $1\frac{5}{8}$-inch diameter and 16 inches focal length; micrometer eyepiece, comb, and wires; mercury bottle and trough.

„ 41. (15) Level collimator with object-glass $1\frac{7}{8}$-inch diameter and 16 inches focal length; stand, rider-level, and fittings.

„ 42. (16) 10-inch reflecting circle, by Troughton, reading by three verniers to 20″; counterpoise stand; artificial horizon with mercury; two tripod stands.

„ 43. (17) Hassler's reflecting circle, by Troughton, with counterpoise stand.

„ 44. (18) 6-inch reflecting and repeating circle, by Troughton and Simms, contained in three boxes, two of which form stands. Circle divided on silver, reading to single minutes; two inside arcs divided to single degrees, 150 degrees on each side; artificial horizon and mercury.

„ 45. (19) 5-inch reflecting and repeating circle, by Lenoir, of Paris.

„ 46. (20) Reflecting circle by Jecker, of Paris, 11 inches in diameter, with one vernier reading to 15″.

„ 47. (21) Box sextant; reflecting plane and level.

„ 48. (22) Prismatic compass, by Troughton and Simms.

„ 49. (23) Mountain barometer.

„ 50. (24) Prismatic compass, by Thomas Jones, mounted with a cylindrical lens.

51. (25) Ordinary $4\frac{1}{2}$-inch compass with needle.

„ 52. (26) Dipping needle, by Robinson.

„ 53. (27) Compass needle, mounted for variation.

„ 54. (28) Magnetic intensity needle, by Meyerstein, of

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Göttingen; a strongly fitted brass box with heavy magnet; filar suspension.

No. 55. (29) Box of magnetic apparatus.

„ 56. (30) Hassler's reflecting circle, by Troughton; a 10½-inch reflecting and repeating circle, with stand and counterpoise, divided on platinum with two movable and two fixed indices; four verniers reading to 10″.

„ 57. (31) Box sextant and glass plane artificial horizon, by Troughton and Simms.

„ 58. (32) Plane 2⅜-inch speculum, artificial horizon, and stand.

„ 59. (33) 2½-inch circular level horizon, by Dollond.

„ 60. (34) Artificial horizon, roof, and trough; the trough 8¼ by 4½ inches: tripod stand.

„ 61. (35) Set of drawing instruments, consisting of 6-inch circular protractor and common protractor, T-square: one beam compass.

„ 62. (36) A pentagraph.

„ 63. (37) A noddy.

„ 64. (38) A small Galilean telescope with object-glass of rock crystal.

„ 65. (39) Five levels.

„ 66. (40) 18-inch celestial globe.

„ 67. (41) Varley stand for telescope.

„ 69. (43) Telescope, with the object-glass of rock crystal.

„ 70. Portable equatoreal stand.

„ 71. Portable altazimuth tripod.

„ 72. Four polarimeters.

„ 74. Registering spectroscope, with one large prism.

„ 76. Two five-prism direct-vision spectroscopes.

„ 78. 9¼-inch silvered-glass reflector and stand, by Browning.

„ 79. Spectroscope.

„ 80. A small box, containing three square-headed Nicol's prisms; two Babinet's compensators; two double-image prisms; three Savarts; one positive eyepiece, with Nicol's prism; one dark wedge.

„ 81. A back-staff, or Davis' quadrant.

„ 82. A nocturnal or star dial.

„ 83. An early non-achromatic telescope, of about 3 feet focal length, in oak tube, by Samuel Scatliffe, London.

„ 84. A Hollis observing chair.

„ 85. Double image micrometer, by Troughton and Simms.

„ 86. 4½-inch Gregorian reflecting telescope, by Short, with altazimuth stand and 6-inch altitude and azimuth circles and two eyepieces.

87. 3¼-inch Gregorian reflecting telescope with wooden tripod stand.

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No. 88. Pendulum with 5-foot brass suspension rod, working on knife edges, by Thomas Jones.
„ 89. A Rhabdological Abacus. A contrivance invented by Mr. H. Goodwyn, consisting of a box filled with compartments, in which are square rods covered with numbers, which can be arranged so as to facilitate the labour of multiplying high numbers.
„ 90. An Arabic celestial globe of bronze, not quite 6 inches in diameter.
„ 91. Astronomical time watchcase, by Professor Chevallier.
„ 92. 2-foot protractor, with two moveable arms, and vernier.
„ 93. Beam compass, in box.
„ 94. 2-foot navigation scale.
„ 95. Stand for testing measures of length.
„ 96. Artificial planet and star, for testing the measurement of a fixed distance at different position-angles.
„ 97. 12-cell Leclanché battery.
„ 98. 2 feet 6 inch navy telescope with object-glass 2½ inches, by Cooke, with portable wooden tripod stand.
„ 99. 12-inch transit instrument, by Fayrer & Son, with level and portable stand.
„ 100. 9-inch transit instrument, with level and iron stand.
„ 101. Small equatoreal sight instrument, by G. Adams, London.
„ 102. Sun-dial, by Troughton.
„ 103. Sun-dial, by Casella.
„ 104. Sun-dial.
„ 105. Box sextant, by Troughton and Simms.
„ 106. Prismatic compass, by Schmalcalder, London.
„ 107. Compass, by C. Earle, Melbourne.
„ 108. Prismatic compass, by Negretti and Zambra.
„ 109. Dipleidoscope, by E. Dent.
„ 110. Abney level, by Elliott.
„ 111. Pocket spectroscope, by Browning.
„ 112. Small brass astrolabe.
„ 113. Double sextant, by Jones.
„ 114. Two models, illustrating the effects of circular motions.
„ 115. A cometarium.
„ 116. A pair of 18-inch globes.
„ 117 } Two old sun-dials, bequeathed by the late W. S.
„ 118 } Heineken, Esq.

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The following instruments are lent, during the pleasure of the Council, to the undermentioned persons:—

No. 4. The *Beaufoy* transit instrument, to the Observatory, Kingston, Canada.
„ 23. The *Matthew* transit, to Captain W. Noble.
„ 74. Registering spectroscope, with prism, to Mr. R. J. Lecky.
„ 76. One five-prism direct-vision spectroscope, to Mr. Talmage.

From the *Sheepshanks* collection:—

No. 27. (1) 30-inch transit instrument and stand, to Mr. G. M. Whipple.
„ 29. (3) Clock and equatoreal mounting, to Mr. R. Bryant.
„ 29. (3) Three micrometers, to Mr. E. Ristori.
„ 30. (4) 3½-inch equatoreal and stand, to Mr. H. Sadler.
„ 31. (5) 2¾-inch telescope and stand, to Mr. E. Ristori.
„ 34. (8) Transit instrument, to Prof. C. Pritchard.
„ 69. (43) Telescope, with rock-crystal object-glass, to Dr. W. Huggins.

The Gold Medal.

The Council have awarded the Society's Gold Medal to Mr. A. A. Common for his photographs of Celestial Bodies. The President will lay before the Society the grounds upon which this award has been founded.

Publications of the Society.

Vol. XLVII. of the *Memoirs* has been published during the past year. It contains the following papers:—

On the Moon's Photographic Diameter, and on the Applicability of Celestial Photography to Accurate Measurement. By Prof. C. Pritchard.

Observations of the Transit of *Venus*, 1874, December 8–9, made in Victoria, New South Wales, South Australia, India, and the Cape of Good Hope. By R. L. J. Ellery, H. C. Russell, J. Tebbutt, C. Todd, Captain Bigg Wither, E. J. Stone, &c.

Measures of Sir John Herschel's Cape Stars, together with a List of New Double Stars. By H. C. Russell.

Double-star Observations made in 1879 and 1880 with the 18½-inch Refractor of the Dearborn Observatory, Chicago. By S. W. Burnham.

On the Solar Motion in Space. By W. E. Plummer.

Photometric Determination of the Relative Brightness of the Brighter Stars North of the Equator. By Prof. C. Pritchard.

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Obituary.

The Council regret that they have to record the loss by death of the following Fellows and Associates during the past year:—

Fellows:—Francis Abbott.
George Bishop.
C. J. Corbett.
W. A. Cross.
J. H. Dallmeyer.
Joseph Drew.
Rev. N. S. Godfrey.
F. C. Green.
Gen. Sir H. Harness.
Julius Page.
Gen. Sir E. Sabine.
Rev. A. Smith.
Prof. H. J. S. Smith.
William Spottiswoode.
Thomas Warner.
T. R. White.

Associates:—Wilhelm Klinkerfues.
J. F. J. Schmidt.
Yvon Villarceau.

Charles Joseph Corbett was born on May 29, 1823, at Mareham-le-Fen, in Lincolnshire, and was the third son of Mr. Joseph Corbett of that place.

By profession, Mr. Corbett was an architect, and it was not till about fifteen years ago that he turned his attention seriously to astronomy. From that time, until advancing years and infirmity of eyesight compelled him to give up work, he occupied himself chiefly in the investigation of solar phenomena, and made a number of observations of Sun-spots, which, however, he was never able to complete. His work was carried on at his own observatory, at Imber Court, Thames Ditton, his residence in Surrey. He died on March 24, 1882, and was buried at Thames Ditton. He was elected a Fellow of the Society on December 10, 1869.

Colonel William Assheton Cross, of Red Scar, near Preston, was educated at Rugby, and Trinity College, Cambridge. He devoted himself from an early age to scientific pursuits, and had constructed a well-appointed Observatory at his residence even in his college days. Here he had mounted equatorially a celebrated 5-foot refractor by Dollond, once the property of the Rev. W. R. Dawes, with which many of that well-known observer's early observations had been made. Later in life Colonel Cross became

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the friend and disciple of the late Mr. Lassell, and under his guidance succeeded admirably in grinding and polishing very excellent specula, one of which, a Newtonian of 15 in. diameter, he mounted equatorially, with all the best appliances, in a second Observatory with hemispherical dome. The figure of this speculum was wonderfully perfect, and elicited the warmest approval from Mr. Lassell. By a rude stroke of fate, one which denotes the rapid strides that have been made in the size of astronomical instruments in the course of one generation, Dawes's refractor mentioned above was degraded to become the finder of the new reflector. An excellent eye and remarkable mechanical skill were conspicuous in Col. Cross, and had not his time and powers been occupied by other matters, he would have earned for himself a high place among amateur astronomers. He died suddenly at the age of 64. He was elected a Fellow of the Society on April 14, 1848.

Joseph Drew, son of the late Joseph Drew, of the Dockyard Service, R.N., was born May 21, 1814, at Deptford. He was founder, proprietor, and editor from 1850 to 1862, of the *Southern Times*, and author of "In the Beginning," "The Mystery of Creation," "Our Home in the Stars," "The Rival Queens," "The Life of the Duke of Wellington," and other essays. He delivered gratuitously nearly 200 lectures, scientific and historical, at various institutions in the south-west of England between 1867 and 1876.

The degree of LL.D. was conferred on him by Richmond University, U.S.A., in 1874. He was a Fellow of the Geological Society, and a member of several other scientific and literary societies. He was elected a Fellow of this Society on January 13, 1871.

Nathaniel Stedman Godfrey was the only child of Nathaniel Godfrey, Esq., of London. After leaving St. Catherine's College, Cambridge, he became curate at Swansea, and subsequently at Biddenham, near Bedford. He then became incumbent of Wortley, near Leeds, and after taking temporary duty at Portsea and Southsea he became the first vicar of St. Bartholomew's Church, Southsea, and occupied this position until his death, which took place, after a long and painful illness, on October 26, 1883, in the 66th year of his age. He married Caroline Ann, the second daughter of Joseph Goodhart, Esq., of Hackney. He was successful as a composer of sacred music, and published several doctrinal works, some of them of a controversial character, and all of the Evangelical type.

Mr. Godfrey possessed a fine Newtonian reflector with silver-on-glass mirror, 9-ft. focus, by Calver, which he had mounted in an Observatory of his own contrivance. In some letters to the *English Mechanic* in 1879–80 he defended reflecting telescopes, pointing out the cause of frequent complaint, and showing

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how he had obtained accurate adjustment in his own case. In one of these letters he mentioned that in the course of forty-five years he had possessed about fifteen achromatic telescopes from $2\frac{1}{4}$ in. up to 7 in.; the object of the letter was to explain how the definition of a fine mirror could be made equal to that of a fine achromatic. He was elected a Fellow of the Society on December 9, 1864.

Frederic Charles Green was born at Bolton, and educated at Bolton and Birkenhead schools. He took the greatest interest in seamanship and nautical matters, both practical and theoretical; and all the time that he could spare from his business of bleaching cotton goods, he devoted to the working of a small sail-boat of his own, and to studying such questions as the law of storms, methods of observing at sea, &c., and he occasionally wrote to the nautical papers on these subjects. To put his knowledge to a practical test, he passed the Board of Trade Examination. It was his great endeavour to assist mates and others engaged in practical seamanship in obtaining the requisite knowledge of the theoretical part of the subject. He was not thirty years of age when he died, of consumption, at the Isle of Man, where he had gone for the sake of his health. He was elected a Fellow of the Society on January 12, 1883.

Sir Henry Drury Harness was a son of John Harness, M.D., Commissioner of the Transport Board, and was born in the year 1804. He was educated at the Royal Military Academy, Woolwich. It was a time of reaction in military affairs, and he felt the effects of this in having to suffer a delay of two years in obtaining his commission. He employed this interval in making an excursion to Mexico, where the silver mines were then attracting the attention of English adventurers. He was appointed second lieutenant in the Royal Engineers, and his first service was at Bermuda, under General Blanshard. In 1832 he was made first lieutenant. He returned to England in 1834, and was appointed an Instructor of Fortification at the Royal Military Academy, Woolwich. At this time he laid the foundations of those papers which were afterwards expanded to form a text-book of Fortification for that school. In 1840 he went to the Engineer School at Chatham, as Instructor in Surveying. At Woolwich and at Chatham he attracted his pupils, both to himself and to the subject of study, by his simple winning manner, and by his patient disposition. He became second captain in 1843. For a short time in 1844 he was Professor of Fortification at the Royal Military Academy, Woolwich. In 1845 he accepted the office of Inspector of Welsh Roads, which was established at that time, in consequence of the change from the system of maintaining them by turnpikes to that of county rates. At this time, the manner of dealing with the new railways became a subject of difficulty to the Board of Trade, and a separate Commission was appointed, of

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which Captain Harness was one of the two Secretaries. It was subsequently merged in the Board of Trade, and he became sole Secretary.

During this time the railway system was developing with great rapidity, and he was therefore brought into contact with all the principal railway engineers of the day. Independently of these duties, he was at the time also acting as arbitrator between the Post Office and the railway companies, a responsible and difficult service, the subject of the arbitration being the remuneration to be paid to the railway companies for the conveyance of the mails. He was next called upon to undertake the duty of reforming the Royal Mint, on the basis of the report of a Royal Commission, which had been appointed to inquire into its constitution and management. The Master of the Mint at that time (1850) was a political officer, whose responsibilities were limited to his Parliamentary duties. Captain Harness was appointed Deputy-Master, and became the responsible head of the department. The mechanical operations connected with the production of coin were then directed and controlled by a quasi-corporation of melters, assayers, and moneyers, who contracted with the Deputy-Master for the execution of the coinage at certain prices; and the task assigned to Captain Harness was to convert this corporation into a Government department. This he successfully accomplished, and he had been virtually appointed Master of the Mint, when, it being considered desirable that this office should be held by an eminent scientific man, his nomination was rescinded, and the appointment given to Sir John Herschel.

He worked loyally under his new chief till the reforms were completed, when he resigned. The Governorship of New Zealand was then offered to him, but he declined it. After this he went as a Commissioner of Public Works to Ireland, and there he remained two years. He returned to England in 1855, to take the position of head of the fortification branch in the office of the Inspector-General of Fortifications, and he held this post during the latter part of the Crimean war. He was then appointed to the command of the Royal Engineers at Malta; but he had not been there long when the mutiny in India broke out, and he was offered the command of the body of Royal Engineers which was to form part of the force under Lord Clyde, and this offer he at once accepted. He had been appointed first captain in 1847, brevet-major in 1854, lieutenant-colonel in 1855. He took part in the operations at Cawnpore against the Gwalior contingent, the siege and capture of Lucknow, and the subsequent engagements in Rohilkund and Oudh. He distinguished himself by personal bravery in the field; and for his services in India he was several times mentioned in despatches. He was also nominated a Companion of the Order of the Bath, and received the medal and clasps.

On his return from India he was appointed, in 1860, the head of what is now the School of Military Engineering at

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Chatham; and under his direction the whole establishment was rearranged in a form more suited to modern military science. In 1866 the cattle plague was giving much anxiety and trouble to the Privy Council; a separate department was created for a time to undertake all that the new Orders in Council required, and General Harness was placed at the head of it, and thus found himself the virtual dictator of the cattle trade of the country. He became major-general in 1868, and in 1877 he was appointed a colonel-commandant, Royal Engineers, and promoted to the rank of lieutenant-general in the army, after which he was placed on the retired list as a general. He became a K.C.B. in 1873. The last duty he undertook was to be President of a Committee on Siege Operations.

He was entrusted by the Ordnance authorities with the editing of a Course of Mathematics for the Royal Military Academy, which was published in 1853, and was till lately used there as a text-book. The late Dr. Peter Barlow, who was his colleague at Woolwich, and with whom he kept up a close friendship afterwards, gave him great assistance in the preparation of this work. His life was one of constant work, but the mere record of his services gives no just idea of the influence he exerted upon those with whom he was brought in contact during his long career.

He married, in 1828, Caroline, daughter of Mr. Thomas Edmunds, of Cowbridge, Glamorganshire; and died on February 10, 1883, at Barton End, Headington, Oxford. He was elected a Fellow of this Society on December 15, 1839.

J. W. L. G.

Julius Page was the only son of Richard Page, Esq., and in early life was associated in business with his father, who was engaged in the Stock Exchange, and was a frequent writer in the *Times* on the currency, under the name of Daniel Hardcastle. In 1834 Julius Page entered the house of Messrs. Baring Brothers, and remained with them as a valued and much respected clerk until failing health compelled his retirement in 1869. He married in 1834, and had one child, a daughter, who survives him. He died at Greenwich on August 6, 1883, in the 83rd year of his age.

Mr. Page was a member of the old Mathematical Society, and became a Fellow of the Royal Astronomical Society when the former Society was merged in it in 1845. Mr. Page occupied himself for many years with conic sections, treated algebraically, but not by the methods of analytical geometry, and he always took great interest in the solution of problems connected with this subject. He was the last surviving member of the old Mathematical Society, who was a true and characteristic representative of its methods and objects as they were in its best days. In the *Monthly Notices* for 1878 (vol. xxxviii. pp. 145–147), in the obituary notice of Dr. Bowerbank, there was given an account by Mr. Page of the Society as he remembered it. He there states that

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he became a member of it in 1823, when it met in Crispin Street, Spitalfields.

Mr. Page was a man of retiring disposition, only seeking enjoyment in his books, his conic sections, and his lathe. He had acquired a large fund of knowledge, which he was always ready to impart to those who sought for it. He was an acknowledged authority on Exchange and Foreign Bills, and wrote much in conjunction with his father on Peel's Bank Act, and also on the Corn Laws. One of the last subjects that he was interested in was paper currency, and his views were published, with those of others, in a volume by the late Lord Overstone.

Sir EDWARD SABINE was the son of Mr. Joseph Sabine of Lewin, and was born in Dublin, on October 14, 1788. The family to which he belonged, said to be of Italian origin, early settled in Normandy, and removed thence to this country. He received his early education at the Royal Military Colleges of Marlow and Woolwich, obtaining a commission as second lieutenant in the Royal Artillery when but fifteen years of age, on December 22, 1803, and receiving his captaincy in 1813. He served in the American War, 1813–1816, and was present in the campaign of 1814 on the Niagara frontier. He commanded the batteries at the siege of Fort Erie, and was favourably mentioned in despatches. On the conclusion of the war he was appointed, on the recommendation of the President and Council of the Royal Society, astronomer of the first expedition in search of the North-West Passage, commanded by Sir John Ross, in 1818. On the return of that expedition, he accompanied, in the same capacity, the second expedition, 1819–1820, commanded by Sir Edward Parry. As the result of his observations he made two communications to the Royal Society, which were published in the *Philosophical Transactions*, one relating to the irregularities observed in the direction of the compass needle consequent upon the attraction of the iron of the ships, and the other to the variations of the magnetic needle and the intensity of the magnetic force during the voyage. In the latter he pointed out the importance of founding a widely-extended series of observations of magnetic disturbances.

In 1821 and 1822 he conducted a series of pendulum experiments for determining the figure of the earth at several stations at or near the Equator, on the coasts of Africa and America in H.M.S. *Pheasant*, and in 1823 he extended the series to Greenland, Spitzbergen, and Norway, in H.M.S. *Griper*. In these voyages he also paid attention to various other questions, such as deep-sea temperature, ocean currents, the measurement of heights barometrically, the influence of the Gulf Stream on the coasts of Europe, &c. His papers on these and other subjects were published in the *Philosophical Transactions*, the *Edinburgh Journal of Science*, and the *Quarterly Journal of Science*.

In 1825 he and Sir John Herschel were appointed the

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British members of a joint commission between the French and English Governments to determine the difference of longitude between the Observatories of Paris and Greenwich by means of rocket signals. In 1827 he was employed in determining by direct observation the difference in the length of the seconds pendulum at Paris and Greenwich; and at the same time he made a series of experiments in order to determine the ratio of the magnetic forces acting on a needle horizontally suspended at these two places. The results of the experiments appeared in the *Philosophical Transactions*, when he also published other papers relating to pendulum observations.

From this time onwards his energies were mainly devoted to researches on terrestrial magnetism, which is the subject with which his name will always be chiefly connected. Numerous memoirs of his, representing an immense amount of work, were published in the *Philosophical Transactions*, the British Association *Reports*, Quetelet's *Correspondance Mathématique*, &c.; in fact, during the forty years from 1830 to 1870 he laboured earnestly and persistently to extend our knowledge of the magnetic condition of the globe, and the present state of this science is mainly due to his untiring efforts. In the British Association *Reports* from 1835 to 1838 he published memoirs relating to the magnetic intensity in the British Isles and over the Earth's surface. By 1838 he had so completely demonstrated the importance of magnetic observations being carried out in all parts of the world, that Capt. James Ross was sent with the *Erebus* and *Terror* to make a magnetical survey of the Antarctic regions, and Sabine accompanied the expedition. He had induced the Admiralty to promote the establishment of observatories in the Colonies, and on the voyage out, magnetical and meteorological observatories were founded at St. Helena, the Cape, and Hobarton (Van Diemen's Land). These observatories were placed under his superintendence, and at this time a general magnetic survey was commenced by him under the direction of the Admiralty. The observatories remained under his control for many years, and the magnetic survey of the globe continued to occupy his chief attention so long as he was capable of work. The results were communicated to the Royal Society in a series of elaborate papers. His last paper appeared in 1872, when he gave a magnetical survey of the North Polar regions. The infirmities of age were then stealing upon him, and he would scarcely have been able to complete it without the assistance of Sir Frederick Evans, the present Hydrographer of the Admiralty. As one of the results of the observations made at the Colonial Observatories, Sabine was led to notice the connection between the number of spots on the Sun and magnetic disturbances. He contributed altogether nearly forty papers to the *Philosophical Transactions*.

In 1826 he married Elizabeth Juliana, daughter of Mr. W. Leeves; she was the companion of his labours, and assisted him in all his work. She died in 1879. He had a great admiration

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of Humboldt, and edited an English translation of his *Cosmos* made by Mrs. Sabine. In 1830, in consequence of the disturbed state of Ireland, where his company of artillery was then stationed, he was required to join it; and he also served there, partly with his regiment and partly on the general staff of the army in Ireland, from 1833 to 1837.

He was elected a Fellow of the Royal Society on April 16, 1818, so that for the long period of sixty-five years he was a Fellow of that Society. His first paper, on the birds of Greenland, was communicated to the Linnean Society in the same year (1818); his contributions to science, therefore, cover no less than fifty-four years. He was Secretary of the Royal Society in 1827, in 1846 Foreign Secretary, in 1850 Vice-President and Treasurer. In 1861 he succeeded Sir Benjamin Brodie as President, and held the office until 1871. He was General Secretary of the British Association from 1839 to 1858, and was President at the meeting in Belfast in 1852. He was elected a Fellow of this Society on November 8, 1839.

He was awarded the Copley medal of the Royal Society in 1821, and a Royal medal in 1849; he also received the Lalande medal of the French Academy. He became lieutenant-colonel in 1851, and major-general in 1859. He was made a K.C.B. in 1869. He was an honorary member or associate of almost every foreign academy and scientific society of note. He died at his residence at London on June 26, 1833, in his ninety-fifth year, and was buried at Tewin beside his wife on June 30. For some time before his death his faculties had failed him. He leaves no family. J. W. L. G.

Henry John Stephen Smith was born on November 2, 1826, and was the fourth child of his parents. His father, John Smith, was a barrister-at-law, and graduated at Trinity College, Dublin, and afterwards at Brasenose College, Oxford, in order to shorten the residence at the Inns of Court that was requisite before he could be called to the bar. At the Temple he was the law pupil of Henry John Stephen, serjeant-at-law, the editor of "Blackstone's Commentaries," and the pupil gave the master's name to the younger of his two sons.

When Henry Smith was just two years old his father died. There were four children, two sons and two daughters, of whom the eldest, a girl, was but nine years of age; and to their education the widow thenceforth devoted herself. Chiefly in order to give her children the better opportunity of education which England afforded, Mrs. Smith left Ireland, and after spending some time in the Isle of Man, at Harborne near Birmingham, and at Leamington, she moved in 1831 to Ryde, in the Isle of Wight, where she remained for nearly ten years. Mrs. Smith was a most accomplished lady, and an intense delight in learning was one of the ruling impulses of her life. She taught her children herself, and until Henry was over eleven he remained

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under her exclusive care and teaching. He was able to read when he was three years of age, and while still between four and five he began to display the desire for learning and the facility for acquiring knowledge that distinguished him ever afterwards. When he was between eleven and twelve his mother, who had been reading Greek plays, Herodotus and Thucydides with the boys, began to feel herself unable to cope with the further difficulties of Latin and Greek composition, and Mr. R. Wheler Bush became their tutor for some months. In a letter to the *Times* of February 12, 1883, Mr. Bush has given an account of the work of his pupil, from which the following is an extract:—

"In the years 1838–39 Henry Smith, then a boy of eleven years of age, read with me for about nine months at Ryde, in the Isle of Wight. He had been previously taught by his widowed mother—a remarkably clever and highly educated woman. After reading with Henry Smith I had a large experience of boys during a head-mastership of more than thirty-three years, but I have often remarked that the brilliant talents of Henry Smith prevented me from ever being really astonished at the abilities of any subsequent pupil. His power of memory, quickness of perception, indefatigable diligence, and intuitive grasp of whatever he studied were very remarkable at that early age. What he got through during those few months, and the way in which he got through it, have never ceased to surprise me. From a record which I have before me I see that during that short time he read all Thucydides, Sophocles, and Sallust, twelve books of Tacitus, the greater part of Horace, Juvenal, Persius, and several plays of Æschylus and Euripides. I see also that he got up six books of Euclid, and algebra to simple equations; that he read a considerable quantity of Hebrew; and that, among other things, he learnt all the Odes of Horace by heart. I could scarcely understand at the time how he contrived at his early age to translate so well and so accurately the most difficult speeches of Thucydides, without note or comment to guide him. He was a deeply interesting boy, singularly modest, lovable, and affectionate."

Mr. Bush was succeeded by two other tutors of less ability, and Mrs. Smith found that adequate teaching for Henry could not be obtained from resident tutors. She accordingly removed to Oxford in the autumn of 1840, when he became the daily pupil of the Rev. H. Highton. In the summer of 1841 Mr. Highton was appointed to a mastership at Rugby. He was accompanied by his pupil, who, being only fifteen, was disqualified by age from entering the sixth form, although possessed of sufficient knowledge. He was placed in the upper fifth and in "the twenty" until the Midsummer holidays of 1842, when having been allowed the privilege of bidding Dr. Arnold goodbye, as a boy who would commence the next term in the sixth form, he returned home, where the news of Dr. Arnold's sudden

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death followed him the next day. He went back to Rugby, and soon became the head boy under Dr. Tait.

On the death of his elder brother from consumption, in September 1843 he was removed from Rugby, and remained with his family at Nice through the winter, almost without classical books and without even a Greek lexicon. He spent the summer of 1844 in Switzerland, and at the beginning of October it was thought that he should return to Rugby for a few weeks' preparation before going to Oxford to try for the Balliol Scholarship in November. He was successful in obtaining the scholarship, and joined his family at Rome before Christmas. The winter was one of great enjoyment to him, and he made rapid progress in knowledge and cultivation while studying diligently the antiquities of the city.

He accompanied his family in June 1845 to Frascati, where in August he was attacked by malaria, the effects of which invalided him for nearly two years. The winter was spent at Naples, the malady slowly wearing itself out. In May 1846 he was taken to Wiesbaden, where the waters rapidly restored him to fair health. It was not, however, thought expedient that he should resume his interrupted course at Oxford until Easter 1847. The intervening winter was spent at Paris, and like the preceding one was a time when he made rapid intellectual progress. He remembered with especial pleasure the lectures of Arago and Milne Edwards. After the Easter term of 1847 he was never to the time of his death absent from Oxford for a single term. In the summer of 1847 he once more visited Wiesbaden, returning with his family to England in October. Until his mother's death, in 1857, his vacations were all passed with his family, the Christmas and Easter vacations chiefly in London, and the summer in Germany, Switzerland, or Austria.

He won the Ireland Scholarship in the Lent term 1848, and obtained a first class both in the classical and mathematical schools in the Lent term 1849. He gained the Senior Mathematical Scholarship in 1851. He was elected a Fellow of Balliol in 1850, and resided in college till 1857, when, after his mother's death, his only surviving sister, Miss Eleanor E. Smith, came to Oxford, and from that time onwards they lived together. He was elected Savilian Professor of Geometry in 1861 in succession to Prof. Baden Powell, and in 1874 he was appointed Keeper of the University Museum. He then removed to the residence attached to the Museum, and lived there with his sister till his death.

After taking his degree he wavered between classics and mathematics, but not for long; the latter soon attracted him, and with a power that steadily increased from year to year. His first two papers, which were geometrical, were published in 1852 in the *Proceedings of the Ashmolean Society* and the *Cambridge and Dublin Mathematical Journal*. His next paper entitled "De Compositione numerorum primorum formæ $4\lambda+1$ ex

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duobus quadratis," was published in *Crelle's Journal* for 1855. This was his first contribution to the subject to which he was to mainly devote his life, and with which his name will ever be associated. For the ten years from 1854 to 1864 he devoted himself to the Theory of Numbers, the most extensive and the most difficult of all the branches of pure mathematics, and made himself master of everything that had been published upon it. The results of this enormous amount of research are contained in his Report on the Theory of Numbers, which appeared in the British Association volumes from 1859 to 1865. Although incomplete, this magnificent Report occupies nearly 250 pages, and forms an enduring monument of the power and genius of its author. It is quite unique of its kind, and presents a comprehensive view of the actual state of the most intricate department of mathematics, and only those who have had occasion to make use of it can appreciate the wonderful grasp exhibited by the author of *all* the processes and methods and literature of the subject. It is a model of brief, clear, and precise exposition, and is remarkable for the same perfection of form, condensed mode of statement of processes, and what may be termed "mathematical good taste" that distinguished all his work.

Not only does the Report contain a complete account of the wonderful series of discoveries of Gauss and his successors, but there is also much original matter, though with characteristic modesty it is but rarely that it is distinguished in any way from theorems that are merely quoted. But the amount of original work that he accomplished was far greater than he could find room for in the Report, and the chief results of his own discoveries were communicated to the Royal Society in two elaborate memoirs on systems of linear indeterminate equations and congruences, and upon the orders and genera of ternary quadratic forms, which were printed in the *Philosophical Transactions* for 1861 and 1867. The two great divisions into which the Theory of Numbers may be divided are the Theory of Congruences and the Theory of Forms, and these papers contain contributions of the highest generality and importance to both branches of the subject. The solution of the problem of the representation of numbers by binary quadratic forms is one of the great achievements of Gauss, and the fundamental principles upon which the treatment of such questions must rest were given by him in the *Disquisitiones Arithmeticæ*. Gauss there added some results relating to ternary quadratic forms, but the extension from two to three indeterminates was the work of Eisenstein, who, in his memoir "Neue Theoreme der höheren Arithmetik," defined the ordinal and generic characters of ternary quadratic forms of an uneven determinant; and, in the case of definite forms, assigned the weight of any order or genus. But he did not consider forms of an even determinant, nor give any demonstrations of his work, and these omissions Prof. Smith supplied in his great memoir on the subject, which affords a complete classification of

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ternary quadratic forms. Prof. Smith, however, did not confine himself to the case of three indeterminates, but succeeded in establishing the principles on which the extension to the general case of n indeterminates depend, and obtained the general formulæ; thus effecting the greatest advance made in the subject since the publication of Gauss's memorable work that so entirely changed the aspect of the whole Theory of Numbers. A brief account of Prof. Smith's methods and results appeared in the *Proceedings of the Royal Society* (vol. xiii., 1864, pp. 199–203, and vol. xvi., 1868, pp. 197–208) in two notices, "On the Order and Genera of Quadratic Forms containing more than Three Indeterminates." In the second of these notices, after giving the general formulæ, Prof. Smith remarks that, though the demonstrations are simple in principle, they require attention to a great number of details with respect to which it is very easy to fall into error, and he adds "so soon as they can be put into a convenient form they shall be submitted to the Royal Society;" but unfortunately he was closely occupied with other researches during all that remained of his life, and when death removed him so suddenly last year they still lay unpublished in his note-books.

In this second notice he also remarks at the conclusion that the theorems which have been given by Jacobi, Eisenstein, and in such great profusion by Liouville, relating to the representation of numbers by four squares and other simple quadratic forms, are deducible by a uniform method from the principles indicated in the paper, and he proceeds, "So also are the theorems relating to the representation of numbers by six and eight squares, which are implicitly contained in the developments given by Jacobi in the 'Fundamenta Nova.' As the series of theorems relating to the representation of numbers by sums of squares ceases, for the reason assigned by Eisenstein, when the number of squares surpasses eight, it is of some importance to complete it. The only cases which have not been fully considered are those of five and seven squares. The principal theorems relating to the case of five squares have indeed been given by Eisenstein (*Crelle's Journal*, vol. xxxv. p. 368); but he has considered only those numbers which are not divisible by any square. We shall here complete his enunciation of those theorems, and shall add the corresponding theorems for the case of seven squares." The class of theorems in question (viz. the number of representations of a number as a sum of squares) was shown by Eisenstein to be limited to eight squares. The solutions in the cases of two, four, and six squares may be obtained by means of Elliptic Functions, *i.e.* by purely algebraic methods, but the cases in which the number of squares is uneven involve the special processes peculiar to the Theory of Numbers. Eisenstein gave the solution in the case of three squares, and he also left a statement of the solution he had obtained in the case of five squares. His results, however, were

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published without demonstration, and only apply to numbers having a particular form. Fourteen years later, in ignorance of Prof. Smith's work, the demonstration and completion of Eisenstein's theorems for five squares were set by the French Academy as the subject of their "Grand Prix des Sciences Mathématiques." When Prof. Smith saw the announcement of the prize subject, in February 1882, his time was engrossed with work relating to Elliptic Functions, and besides having a great dislike to become a competitor, especially under the circumstances, he was very reluctant to leave, even for a short time, the work he had in hand. At the request, however, of a member of the Commission by whom the prize had been proposed, he undertook to write out the demonstration of his general theorems so far as was required to prove the results in the special case of five squares. The dissertation was sent in by the day appointed (June 1, 1882), and only a month after his death, in March 1883, the prize of 3,000 francs was awarded to him, another prize being also awarded to M. Minkowski. No episode could bring out in a more striking light the extent of Prof. Smith's researches, or the distance he had advanced beyond his contemporaries than that a question of which he had given the solution in 1867 as a corollary from general formulæ which governed the whole class of investigations to which it belonged should have been regarded by the French Academy as one whose solution was of such difficulty and importance as to be worthy of their great prize. It affords, too, a singular illustration of the little attention that works destined to become classical attracted in the lifetime of their author. In the Royal Society's *Proceedings* Prof. Smith's statement of his results relating to five squares and seven squares occupy but a single page, but his dissertation (which has been printed by the French Academy), although it only relates to the case of five squares, occupies 72 pages. Many of the propositions contained in it are general, but the demonstrations are not supplied for the case of seven squares.

Prof. Smith was also the author of important papers in which he succeeded in extending to complex quadratic forms many of Gauss's investigations relating to real quadratic forms.

He was led by his researches on the Theory of Numbers to the Theory of Elliptic Functions; and on this subject he has published since 1864 results scarcely inferior in importance to his achievements in the former theory. His third subject was Modern Geometry, in which he was without a rival in this country, and of which he showed the same perfect mastery. Pure mathematics is divisible into two great branches, the Theory of Numbers, or "Arithmetic," *i.e.* the theory of discrete magnitude, and Algebra, the theory of continuous magnitude, the aims and methods of the two subjects being quite distinct. A characteristic of Prof. Smith's work, no less than of Gauss's, is the "arithmetical" mode of treatment that runs through the whole of it, no matter what the subject; and his

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great command over the processes of this science is everywhere conspicuous. Special reference ought to be made to one paper of his in the *Atti* of the Accademia dei Lincei for 1877, in which he established a very remarkable analytical relation connecting the modular equation of order n and the theory of binary quadratic forms belonging to the positive determinant n. In this paper the modular curve is represented analytically by a curve in such a manner as to present an actual geometrical image of the complete systems of the reduced quadratic forms belonging to the determinant, and a geometrical interpretation is given to the ideas of *class*, *equivalence*, and *reduced form*. His papers on Elliptic Functions and the higher singularities of curves were published chiefly in the *Proceedings of the Mathematical Society*. At the time of his death he was engaged on a memoir on the "Theta and Omega Functions," which he left nearly complete, 150 quarto pages of it being in type; the latter portion, however, had not been revised in proof by him. This was the memoir which he was forced to lay aside for a time, in order to write out from his notes of 1866–67 the dissertation on the problem of five squares for the French Academy.

In 1868 he was awarded the Steiner prize of the Berlin Academy for a geometrical memoir "Sur quelques Problèmes cubiques et biquadratiques." He was asked to contribute to the memorial volume to Chelini, published at Milan in 1881 by Cremona and Beltrami, and he wrote a paper in Latin on continued fractions. He also wrote the introduction to the collected edition of Clifford's mathematical papers (1882).

Prof. Smith's collected mathematical works will be issued by the Oxford University Press in two volumes quarto. His lectures on Geometry and Theory of Numbers will also be published as text-books, the former being reproduced from the notes of pupils who attended them.

In the early years of his work he had published his researches but sparingly, and it was only as the mass of results accumulated that the necessity for publication pressed itself upon him. The excellence and completeness that distinguished all that Gauss ever published was a characteristic of Prof. Smith's work, and, as in Gauss's case, it was the result of extreme thought and care and elaboration. He was a great admirer of Gauss, and the following words of his relating to Gauss are quoted here in their entirety, as they perfectly describe, and in his own language, the objects which Prof. Smith himself so successfully kept in view in all that he wrote:—

"If we except the great name of Newton (and the exception is one which Gauss himself would have been delighted to make), it is probable that no mathematician of any age or country has ever surpassed Gauss in the combination of an abundant fertility of invention with an absolute rigorousness in demonstration, which the ancient Greeks themselves might have envied. It may be admitted, without any disparagement to the eminence of such

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great mathematicians as Euler and Cauchy, that they were so overwhelmed with the exuberant wealth of their own creations, and so fascinated by the interest attaching to the results at which they arrived, that they did not greatly care to expend their time in arranging their ideas in a strictly logical order, or even in establishing by irrefragable proof propositions which they instinctively felt, and could almost see, to be true. With Gauss the case was otherwise.

'It may seem paradoxical, but it is probably nevertheless true, that it is precisely the effort after a logical perfection of form which has rendered the writings of Gauss open to the charge of obscurity and unnecessary difficulty. The fact is that there is neither obscurity nor difficulty in his writings as long as we read them in the submissive spirit in which an intelligent schoolboy is made to read his Euclid. Every assertion that is made is fully proved, and the assertions succeed one another in a perfectly just, analogical order; there is nothing so far of which we can complain. But when we have finished the perusal we soon begin to feel that our work is but begun, that we are still standing on the threshold of the temple, and that there is a secret which lies behind the veil, and is as yet concealed from us. . . . No vestige appears of the process by which the result itself was obtained, perhaps not even a trace of the considerations which suggested the successive steps of the demonstration. Gauss says, more than once, that for brevity he only gives the synthesis, and suppresses the analysis of his propositions. *Pauca sed matura* were the words with which he delighted to describe the character which he endeavoured to impress upon his mathematical writings. If, on the other hand, we turn to a memoir of Euler's, there is a sort of free and luxuriant gracefulness about the whole performance which tells of the quiet pleasure which Euler must have taken in each step of his work; but we are conscious, nevertheless, that we are at an immense distance from the severe grandeur of design which is characteristic of all Gauss's greater efforts. The preceding criticism, if just, ought not to appear wholly trivial, for though it is quite true that in any mathematical work the substance is immeasurably more important than the form, yet it cannot be doubted that many mathematical memoirs of our own time suffer greatly (if we may dare to say so) from a certain slovenliness in the mode of presentation, and that (whatever may be the value of their contents) they are stamped with a character of slightness and perishableness which contrasts strongly with the adamantine solidity and clear, hard modelling which (we may be sure) will keep the writings of Gauss from being forgotten long after the chief results and methods contained in them have been incorporated in treatises more easily read, and have come to form a part of the common patrimony of all working mathematicians. And we must never forget (what in an age so fertile of new mathematical conceptions as our own we are only too

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apt to forget) that it is the business of mathematical science not only to discover new truths and new methods, but also to establish them, at whatever cost of time and labour, upon a basis of irrefragable reasoning.

"The *μαθηματικὸς πιθανολογῶν* has no more right to be listened to now than he had in the days of Aristotle; but it must be owned that since the invention of the 'royal roads' of analysis, defective modes of reasoning and of proof have had a chance of obtaining currency which they never had before. It is not the greatest, but it is perhaps not the least, of Gauss's claim to the admiration of mathematicians, that while fully penetrated with a sense of the vastness of the science, he exacted the utmost rigorousness in every part of it, never passed over a difficulty as if it did not exist, and never accepted a theorem as true beyond the limits within which it could actually be demonstrated."

These words describe not only Prof. Smith's views but the quality of his own work. His one care was that it should be imperishable; and the words "adamantine solidity" express better than any others could do the character of what he has left. He spared no time or pains that his work should be as complete in its details as in its main results, and that it should be as perfect in form as in substance. He wished that what he did should be done for all time, and that it should also receive from his own hand the form which it was to retain.

He was President of Section A of the British Association at Bradford in 1873 and of the Mathematical Society in 1874–76, and the presidential addresses which he delivered at Bradford and on vacating the latter office are masterpieces of graceful writing that bear record to his wonderfully extensive knowledge and rare combination of gifts. As an expounder of mathematics before an audience he was unsurpassed for clearness, and his singular charm of manner gave him a remarkable power of fixing the attention of those present. Perhaps his faultless method of exposition and peculiar grace of manner were never more apparent than at the Mathematical Society on December 12, 1882—the last occasion on which he was ever to lay before a society the results of his wonderful power of penetration into the innermost structure, as it were, of mathematical truth.

His contributions to general literature were not numerous; a memoir of his friend Prof. Conington and an essay on the Plurality of Worlds were the most important. The articles on Arithmetical Instruments and on Geometrical Instruments and Models which he wrote for the handbook published in connection with the Special Loan Exhibition of Scientific Apparatus at South Kensington in 1876, should also be noticed.

He was appointed a member of the Royal Commission on Scientific Instruction and the Advancement of Science in 1870 on the death of Prof. W. A. Miller; and he was also a member of the Oxford University Commission, appointed in 1877. On

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both Commissions a great deal of very heavy work was performed by him; and knowing how short was to be the time for which he was to be spared, it is impossible not to regret the great amount of time and anxiety that were thus exacted from him. A considerable portion of the report of the former Commission, including the part which related to the Universities of Oxford and Cambridge, was drafted by him.

In 1877, on the reconstitution of the managing body of the Meteorological Office, as recommended by the Treasury Commission in 1876, Prof. Smith was requested to accept the chairmanship of the new council. This office, which involved weekly visits to London, in addition to council meetings, which usually took place once a fortnight, he still held at his death. During the five years and a half of his presidency, he never missed a meeting, and he always devoted at least two hours to the consideration of the business on the agenda paper. The Minutes were always revised by him in manuscript before they were printed. In 1879 he was requested by the Council to attend the International Meteorological Congress at Rome, where his wide and varied knowledge and great personal influence made themselves felt throughout the whole meeting and contributed greatly to its success.

He was elected a Fellow of the Royal Society in 1861. On April 12 of the same year he was elected a Fellow of this Society. He was an LL.D. of Cambridge and Dublin.

It is difficult to give an idea of the position Prof. Smith held in Oxford and in society generally, so brilliant were his attainments, and so great and so varied his personal and social gifts. In an article on him in the *Times* of February 10, 1883, it was truly said: "It is probable that of the thousands of Englishmen who knew Henry Smith, scarcely one in a hundred ever thought of him as a mathematician at all . . . He was a classical scholar of wide knowledge and exquisite taste, and there were few who talked to him on English, French, German, or Italian literature who were not struck by his extensive knowledge, his capacious memory, and his sound critical judgment." The writer might have added that, even of the few who did think of him as a mathematician, there were fewer still who had any suspicion of the place he held in mathematical science, or of his intense devotion to the subject. The following extracts from the *Spectator* of February 17 will help to convey some impression of his personal character: "Many even of those who are aware that a man in the fulness of his powers is just dead, whose brilliant intellectual attainments have probably not been surpassed by any other of their English contemporaries may, nevertheless, be surprised at regret so widely felt and so loudly expressed over the loss of one who wrote no great books, patented no great invention, amassed no fortune, made no famous speeches, and led no conspicuous movement, political or social. Measured by the popular measure of publicity and fame, Prof. Henry Smith would

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hardly seem to most of us to have been one of the great men of the time. Yet it would be difficult, among the world's celebrities, to find one who in gifts and nature was his superior... His mental attainments were of the highest order. A finished classical scholar, a mathematician, in some respects of European distinction, a considerable metaphysician, a trained master of most branches of knowledge, literary, economic, and scientific, an adequate linguist, and a man of sound judgment, perfect temper, and wise aptitude for affairs, he combined with his other special excellences a delicate gaiety of spirit, a brilliant conversational power, which made him one of the most accomplished and attractive ornaments of any educated company in which he moved. . . . Vanity or self-seeking, every form of mental intemperance and extravagance, seemed to have no place in anything that he ever said or did... Among the many friends, acquaintances, admirers, whose thoughts have in the last few days been saddened or sobered by the unexpected death of a brilliant man of genius, there are none who will not readily accord to Prof. Henry Smith the tribute of unaffected respect for what, without extravagance, may be termed his extraordinary powers of mind, his gentle and Lælian wisdom, and the sweetness of character which never made an enemy, lost a friend, or sought a personal advantage for itself." Referring to his influence at Oxford the article proceeds: "For nearly thirty years no more attractive, brilliant, or genial figure was to be found in the perturbed society of the University. Some happy combination of judgment and temper made him acceptable even to those with whose opinions he had nothing in common. He had the great art of never pressing a victory home, and of bearing defeat with pleasant equanimity. His business powers, his modesty, his wisdom, and his entire freedom from egotism and dogmatic presumption, a delicate gaiety that never flagged, wit that sparkled without wounding, and which rose incessantly to real brilliancy, made him not merely an effective personage in the Oxford world, but universally acceptable in any society, whatever the shade of its opinions . . . As by degrees his attainments were recognised, both in England and abroad, his influence at Oxford naturally deepened, but neither within nor without the University did he grasp at opportunities of notoriety. Such power and authority as he possessed he held without an effort, without solicitation, apparently without any personal satisfaction in them. In offices of friendship he was constant; in such public or civic duties as came in his way, assiduous; no good or benevolent work ever needed a helping hand but his was at its service, without ostentation and without any expectation of personal advantage." The notice of him in the *Athenæum* closed with the words: "No one, probably, has ever had a larger circle of private friends to lament his loss. He had all the gifts which win and preserve attachment; not only sincerity, constancy, depth of feeling and liveliness of sympathy, but a sweetness and nobility of nature to which no words can render adequate testimony."

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In commenting on his nomination as one of the Oxford University Commissioners, Mr. Grant Duff said in the House of Commons: "The Savilian Professor of Geometry is not merely in the first rank of European mathematicians, but he would be a man of very extraordinary attainments even if you could abstract from him the whole of his mathematical attainments. He was the most distinguished scholar of his day at Oxford . . . But Prof. Smith's extraordinary attainments are the least of his recommendations for the office of Commissioner. His chief recommendations for that office are the solidity of his judgment, his great experience of Oxford business, his services on the Science Commission, and his conciliatory character, which has made him perhaps the only man in Oxford who is without an enemy, sharp as are the contentions of that very divided seat of learning." Those who knew Prof. Smith will feel that even these quotations fail to convey any adequate idea of the extent and brilliance of his attainments and powers.

The amount of time and thought that he devoted to the Royal Commissions, Boards of Delegates at Oxford, the Meteorological Office, and Committees and Councils of Societies, was very great, and it seems almost incredible that one who lived so active and busy a life, passed in the midst of men and affairs, should have been the author of mental achievements in the most abstract and complicated of the sciences, which will rank as scarcely second to any in the century. His influence and position he owed to his personal qualities alone, and many of those who thought they knew him best had the least idea how incessantly he was occupied with mathematical researches. The cold severity of his mathematical writings forms a curious contrast to the brilliant gaiety of his manner, and future generations, who will know him only from his writings, will find it hard to believe what they will find recorded of their author. In the character of his work he closely resembles Gauss, but no two lives could be more different.

An accident to his leg confined him to his sofa during part of 1882, but he seemed to have nearly recovered from its effects when he was seized with the illness which so soon carried him off. He was a regular attendant at the Council Meetings of this Society, and, at the time of his death, was a Vice-President. At the meeting of Council on February 2, 1883, the writer of this notice received from him at Burlington House a letter—almost the last which he ever wrote—in which he said: "I cannot be at the R. A. S. to-morrow. It is no fault of mine . . . I shall see you on the ninth, I hope." The ninth was the date of the Anniversary Meeting, but at seven o'clock on that morning he died, and the first words spoken by the President, on taking the chair, were to announce to the Fellows the loss they had just sustained. He was buried at Oxford on February 13: conspicuous among those who stood by his grave was Mr. Spottiswoode, himself so soon to be removed.

J. W. L. G.

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WILLIAM SPOTTISWOODE was born in London on January 11, 1825. He belonged to an ancient Scottish family, many members of which have risen to distinction in Scotland and in America. He was first sent to a school at Laleham, under Mr. Buckland, brother of Dean Buckland. Thence he was removed to Eton, where his stay was short. He was transferred from Eton to Harrow, then under Dr. Wordsworth, his house tutor being Mr. Harris. On entrance he was placed on the "upper shell," a high form in those days for a new comer. At school he was a very studious, quiet, and thoughtful boy, not much given to athletic games. He remained at Harrow three years, and in 1842 obtained a Lyon scholarship. In the same year he entered Balliol College, Oxford, the present Bishop of Exeter (Dr. Temple) being his mathematical tutor. Subsequently, in 1845, the last year of his residence as an undergraduate, he read with the Rev. Bartholomew Price, now Sedleian Professor of Natural Philosophy at Oxford. He took a first class in mathematics, and afterwards obtained the Junior (1846) and Senior (1847) University Mathematical Scholarships. After his degree he resided at Oxford for a short time, and gave a course of lectures at Balliol College on Geometry of Three Dimensions. After leaving Oxford, in 1846, he immediately took an active part in the working management of the business of the Queen's Printers, which was relinquished to him by his father, Andrew Spottiswoode. He possessed remarkable gifts as an organiser, and the business developed largely under his hands. In 1861 he married the eldest daughter of the late William Urquhart Arbuthnot, a distinguished member of the Indian Council.

Spottiswoode's first published work was a small quarto volume of 136 pages, containing five tracts upon different subjects in pure mathematics. This volume appeared in 1847, under the title *Meditationes Analyticæ*. Four years later he published a much more notable pamphlet, entitled *Elementary Theorems relating to Determinants*. A second edition, rewritten and much enlarged, was published in Vol. li. of *Crelle's Journal*. This was the earliest elementary treatise on a useful mathematical notation which, with the algorithm and processes connected with it, has since risen to great importance, and has influenced every branch of Mathematics, and many of its applications. It contains a good account of what had at that time been effected on this subject, and is the best known of Spottiswoode's mathematical writings.

He was the author of numerous papers, chiefly relating to geometry treated analytically, and to symbolic methods, which were printed in the publications of the Royal Society, the *Proceedings of the London Mathematical Society*, the *Philosophical Magazine*, the *Quarterly Journal of Mathematics*, *Crelle's Journal*, the *Comptes Rendus*, the *Proceedings of the Royal Institution*, &c. He was the author of but one paper published by this Society: it appeared in Vol. xxix. (1861) of the *Memoirs*, under the title "On a Method for Determining Longitude by means of Observations on the Moon's Greatest Altitude."

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He contributed to the *Journal of the Royal Asiatic Society* two papers, the one relating to the supposed discovery of the principle of the differential calculus by an Indian astronomer, and the other to the Súrya Siddhánta and the Hindoo method of calculating eclipses.

In 1856 he made a journey through Eastern Russia, and of this he published an account in his book *A Tarantasse Journey through Eastern Russia in the Autumn of* 1856 (London, 1857).

Since 1870 he divided his attention between mathematics and physics; and the papers on electrical discharges through rarefied gases, published by him alone, and also conjointly with Mr. J. F. Moulton, in the *Philosophical Transactions*, are well known.

The prevailing character of his mathematical work may be said to be *symmetry*: and he has been called, like Serret, "un artiste en formules." He was most attracted by what may be called the morphology of mathematics, as, for example, results deduced from the *form* of the expressions. The following words of his own are worth reproducing here, as they not only point out the influence of such matters on the progress of science, but also indicate the kind of work that interested him the most. "There are a few certain relations so elementary in their conception, yet so universal in their application, that they seem capable of forming the basis of extensive theories; such, for example, in geometry is that of anharmonic ratio. Such, again, in algebra are those of homogeneity and of symmetry, which prove to be not merely improvements in form but actually new powers for progress in the hands of the mathematician. The calculus of homogeneous forms has marked a new era in the history of algebra; the theory of equations has been transfigured in its light; mechanics, both ordinary and molecular, have been elucidated by it; and the remote applications of the integral calculus have felt its ever-extending influence. Under these, as it were, new fundamental conceptions whole theories may be coordinated; and of these, again, perhaps some coordination may one day be contemplated. As another instance of this generalisation of principles, and of this dual aspect of the principles so generalised within almost the present generation, it has been discovered, or at all events been duly realised, that symbols of operation combine according to definite laws, comprising as a particular case those of ordinary number. This fertile idea has, year by year, been receiving fuller developments, till it has at last assumed the form of a complete calculus."

His Presidential Address to the London Mathematical Society, which related to the modern algebras that have so distinguished the progress of mathematics in our day, attracted considerable attention among those interested in symbolic processes and symbolic logic. His British Association Address at Dublin, in 1878, related mainly to the philosophy of mathematical theories which depend essentially upon the modern ideas which have grown up so recently in connection with the theory of space.

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He showed great ability as an organiser wherever he had to do with business in any form; and he had in the highest degree the faculty of warmly attaching to himself friends and acquaintances. He was a many-sided man, and it was the combination in him of various gifts and powers, and not the prominence of any one in particular, that won for him so distinguished a position among the men of his time. As a business man he possessed a wonderful capacity for mastering the most complicated detail. He was a man of the world, with a very wide circle of friends, chief among whom were the most earnest and most devoted labourers in different departments of knowledge. His house at Grosvenor Place was the centre of scientific society in London, and his garden parties at his country estate near Sevenoaks were brilliant gatherings of men eminent in various walks of life.

He was Treasurer of the British Association from 1861 to 1874, of the Royal Institution from 1865 to 1873, and of the Royal Society from 1871 to 1878. In 1871 he succeeded Dr. Bence Jones as Honorary Secretary of the Royal Institution. He was President of the Mathematical and Physical Section of the British Association in 1865, of the British Association itself in 1878, and of the London Mathematical Society in the two years 1870–72. He was elected President of the Royal Society in 1878, in succession to Sir Joseph Hooker, and still held this office at the time of his death. He was an LL.D. of Cambridge, Dublin, and Edinburgh, and a D.C.L. of Oxford. He was elected a Fellow of this Society on April 7, 1852. He died on June 27, 1883, of Roman, or typhoid, fever, which he had caught during a visit to Italy. From the first his illness had caused serious alarm; and the more so as his strength had been materially affected by a severe tricycle accident which he met with in the previous year, and as his indefatigable attention to duties of various kinds had greatly overtaxed his constitution. Since Sir Joseph Banks, no President of the Royal Society has died in office. He was buried in Westminster Abbey on July 5, in a grave near that of his ancestor, Archbishop Spottiswood.

In his Presidential Address to the Royal Society on November 30, 1883, Prof. Huxley, referred to his predecessor as a chief, "of whom all those who had the highest interests of this Society at heart hoped that he would continue for many a year to discharge the responsible and laborious duties of his office with that broad intelligence, that faithful diligence, that inexhaustible patience and courtesy, which were so characteristic of the man," and he proceeded as follows:

"Every one of the Fellows of the Society in whose hearing I speak, knows that these are no words of a conventional eulogy, as of a customary epitaph. But it is only those who worked with our late President in the Council, or as officers of the Society, who are in a position fully to appreciate his singular capacity for the transaction of business with clear judgment and rapid decision, and yet with the most conscientious consideration of the

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views of those with whom he was associated. And I may add that it is only those who enjoyed Mr. Spottiswoode's intimate friendship, as it was my privilege to do for some quarter of a century, who can know how much was lost when there vanished from among us that rare personality, so commingled of delicate sensitiveness with marvellous self-control, of rigid principle with genial tolerance, of energetic practical activity with untiring benevolence, that it always seemed to me the embodiment of that exquisite ideal of a true gentleman which Geoffrey Chaucer drew five hundred years ago. . . . It is not for me to pass judgment upon Mr. Spottiswoode's scientific labours; but I have the best authority for saying that, having occupied himself with many branches of mathematics, more especially with the higher algebra, including the theory of determinants, with the general calculus of symbols, and with the application of analysis to geometry and mechanics, he did excellent and durable work in all; and that in virtue of his sound and wide culture, his deep penetration, and the singular elegance with which he habitually treated all his subjects, he occupied a place in the front rank of English mathematicians.

"The interment in Westminster Abbey of one who, though compelled to devote a large share of his time to business, was a born man of science, and had won himself so high a place among mathematicians, was doubtless grateful to us as men of science, . . . yet as men, I think it is good to regard those solemn and pathetic obsequies as the tribute which even our busy, careless, cynical, modern world spontaneously pays to such worth and wisdom, to such large humanity and unspotted purity, as were manifested in the 'very perfect gentle knight' who so well represented the chivalry of science."

Perhaps no words could more happily express the estimation in which Mr. Spottiswoode was held by his friends.

J. W. L. G.

Thomas Warner was born at Leyton, Essex, on June 16, 1822, and died suddenly at Brighton on January 3, 1883. He was a younger son of the late Edward Warner, of Walthamstow, Essex, and was educated at Totteridge Park School and Trinity College, Cambridge, where he graduated in 1846. He married, in 1847, Elizabeth, youngest daughter of Richard Peckover Harris, Esq., of Walthamstow. In 1876 he published a work entitled "How to keep the Clock right by Observations of the Fixed Stars with a small fixed Telescope. Together with Tables of Stars arranged to show, by the use of a little arithmetic, the mean solar time of their apparent transits to the end of the century." This was an improved edition, the first edition having appeared in 1869. He also took considerable interest in mathematical tables and arithmetical calculations.

Mr. Warner was resident for more than thirty years at Brighton, and held the position of a justice of the peace on the

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local bench of magistrates for many years. He will be remembered in Brighton for his active and philanthropic life and his many and munificent charities. He was a liberal supporter of the Sussex County Hospital, and had attended a meeting of its Board on the morning of the day he died. He was elected a Fellow of the Society on January 11, 1867.

Thomas Reynolds White died at his residence, Killingworth House, Northumberland, on May 29, 1883, after a long illness, caused by paralysis, aged 70 years. He was descended on his mother's side from an ancient Northumbrian family (the Killingworths of Killingworth), and inherited with other relatives a portion of the old lands. He possessed a taste for scientific pursuits, but had little time at his disposal to devote to them, being for many years fully engaged in business in London, as a manufacturing chemist, at Castle Street, Saffron Hill, until illness compelled him to discontinue his active interest in it. He served as one of the Local Commissioners for Finsbury for the Exhibition of 1851. He had been a member of the old Mathematical Society from his youth, on the introduction of his father, who had also been a member from early years; and he became a Fellow of this Royal Astronomical Society, with the other members of the Mathematical Society, in 1845.

Antoine François Joseph Yvon Villarceau was born at Vendôme, Loir-et-Cher, on January 15, 1813. After having completed his education, so far as was attainable in the college of his native town, he proceeded to Paris, soon after the Revolution of 1830, full of enthusiasm for literature, art, and music. Here he continued his studies at the Conservatoire with so much success that before leaving in 1833 he obtained a first prize. Having adopted with great fervour the peculiar doctrines of Saint-Simon, he, in the same year that he quitted the Conservatoire, accompanied Félicien David to Egypt as a member of a mission of that sect, at that time stationed there under the direction of M. Enfantin. From his association with the engineer officers of that mission, especially with his friend Lambert Bey, he first obtained a taste for those branches of mathematical and mechanical science in which he afterwards became so distinguished. Returning to Paris in 1837, he became a successful student at the École Centrale, where he remained three years.

Possessed of independent means sufficient to remove all necessity for seeking immediate employment, M. Villarceau, after completing his education at the École Centrale, devoted nearly all his time to the study of analysis and geometry, so that he might have readily at hand the means of investigating the higher questions in mechanics and astronomy. As the first result of this continuous study, he, in 1845, presented a memoir to the Académie des Sciences, entitled, "Méthode de correction des éléments approchés des orbites des comètes au moyen de trois

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observations," an abstract of which is inserted in the *Comptes Rendus*, vol. xx., and afterwards more fully in the *Receuil des Savants Etrangers*. This paper was received favourably by astronomers, and especially by Arago, who, struck with its originality, offered M. Villarceau the post of aide-astronome, as he had previously done to other young rising astronomers who had similarly distinguished themselves. In 1846 M. Villarceau became, therefore, a member of the staff of the Paris Observatory, to which he remained attached to the end of his life. In 1854, soon after the appointment of M. Le Verrier to the directorship of the Observatory, he was promoted to the higher and more permanent rank of astronome-titulaire, which, though not interrupting his usual duties, was the means of giving him greater facilities for continuing his mathematical researches in astronomy, geodesy, and mechanics. At this time, and for several years afterwards, he was accustomed to take a share in the ordinary daily observations, especially with the transit instrument of Gambey. He also made some observations of comets with the Equatorial, including those of Brorsen, Tempel (1859) and others.

Among the first of M. Villarceau's theoretical memoirs which attracted attention out of France were those relating to his researches on the relative orbits of double stars. Fully recognising the important labours of Savary, Encke, and Sir John Herschel on this subject, he attacked the problem with great vigour, taking advantage of the accumulated materials of former observers to calculate accurately, as examples, the orbits of several important binary systems. The first memoir connected with these researches was presented to the Académie des Sciences early in the year 1849; and it gives a mathematical explanation of the method adopted by M. Villarceau in the calculation of the orbits of several double stars, notes on which were communicated to the Academy at various times during the same year. In obtaining materials for these investigations he was greatly assisted by the MM. Struve, who liberally supplied him with several series of unpublished observations, which enabled him to calculate, by his method, the elements of the orbits of ζ *Herculis*, η *Coronæ*, and ξ *Ursæ Majoris* with great confidence. These valuable memoirs and notes are contained in the Appendix to the *Connaissance des Temps* for 1852. This Appendix also contains an elaborate paper on a "Méthode pour calculer les éléments des Planètes, ou, plus généralement, des Astres dont les orbites sont peu inclinées à l'ecliptique, fondée sur l'emploi des dérivées relatives au temps, des trois premiers ordres de la longitude géocentrique, et du premier ordre de la latitude." A more extended memoir, "Détermination des orbites des Planètes et des Comètes," is given in Vol. III. of the *Annales de l'Observatoire de Paris*. The principle of the method adopted in this investigation "is the same as that used by Laplace in his solution of the same problem; but M. Villarceau has pointed out, and skilfully discussed, the various circumstances which render a modification of

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the form of solution indispensable, and has given convenient formulæ applicable to each particular case. He has also shown how the elements may be rectified, by a process of approximation founded on applying successive corrections to the coefficients of the expressions for the geocentric longitude and latitude, the values of these corrections being obtained by the solution of a system of linear equations." (*Monthly Notices*, vol. xix. p. 42.)

M. Villarceau was not slow to apply his revised method in the calculation of the elements of the orbits of several of the minor planets and comets. Elements and ephemerides of the planets *Amphitrite*, *Hebe*, *Iris*, and *Victoria* were computed with great care, and distributed among astronomers, by whom they were found to be of great value for the comparison of the tabular places with the observations of those planets. In calculating his normal positions, M. Villarceau was careful to use as many observations as he could possibly obtain, as may be gathered from the following extract from one of his notes on the planet *Victoria*. He remarks:—"To obtain still greater accuracy, I have represented graphically the comparisons published by MM. Hartnup, Carrington, Vogel, and Graham, along with those of the Observatories of Paris and Cambridge. I have also deduced comparisons from three months' observations made at Washington and collected by Mr. Ferguson. In this way I have constructed nearly 200 ordinates in Right Ascension and as many in Declination, representing the excess of observation above the computations of the ephemeris." Altogether, M. Villarceau has computed about twenty series of elements and ephemerides of the minor planets and comets. He was the first to determine the periodicity of the comet discovered by D'Arrest on June 27, 1851; and it was by the aid of an approximate ephemeris communicated by him to Mr. Maclear that this comet was detected at the Cape Observatory in December 1857, on its first predicted return to perihelion. In July 1861 M. Villarceau presented to the Academy a second series of investigations on the orbit of D'Arrest's Comet. Of other comets whose orbits have been calculated by him, the details of which are given in the *Comptes Rendus*, we need only name those of Petersen, Comet V. 1857, and the great Comet of 1858 (Donati).

In the *Comptes Rendus* for 1872, M. Villarceau has a memoir, entitled, "Sur la Constante de l'Aberration et la Vitesse de la Lumière considerée dans leurs rapports avec le mouvement absolu de translation du système solaire," in which he introduces into the theory of aberration the proper motion of the solar system, and points out that four determinations of the coefficient of aberration relative to four stars, not all situated on the same circle of the sphere, are sufficient theoretically to determine the three coordinates of the motion of the solar system, and practically to fix within the maximum limit the velocity of the Sun in space.

The Catalogue of Scientific Papers of the Royal Society,

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to 1873, gives the titles of seventy-three separate papers of M. Villarceau. He is the author, however, of many more—probably more than a hundred in all; but they are by no means confined to astronomical subjects. In his mechanical papers he gives abundant evidence of his high mathematical power, in illustration of which we need only refer to his memoirs on "Arches de Pont"; "On the Theory of the Stability of Locomotives in Motion"; "On the Analytical Theory of Foucault's Gyroscope"; and to his researches on the rates and compensation of chronometers. In this last memoir (Vol. VII. of the *Annales de l'Observatoire*) he apparently shows that it is not difficult to determine by trial the six coefficients of the expression which represents the rate of a chronometer more or less compensated. His method for correcting chronometers for errors of compensation, described in this memoir, has been confirmed by Commandant De Magnac, after twelve years' comparison at sea, the details of which are given in "Traité de nouvelle Navigation astronomique."

One of the most important works undertaken by M. Le Verrier during his superintendence of the Paris Observatory, was the galvanic determination of the difference of longitude between Paris and important stations in France and neighbouring countries. That distinguished astronomer had not been in office many months when, in conjunction with the Astronomer Royal, he commenced this series of longitude operations, by determining the difference of longitude between the Observatories of Paris and Greenwich. Though M. Villarceau had very little to do with this first determination, it appears that the discussion and preparation of many of those which followed were intrusted to him, as may be seen on reference to Vol. VIII. of the *Annales*. In this volume are given full details of the operations carried on in the years 1861 to 1865, connected with the astronomical determinations of the longitudes, latitudes, and azimuths of Dunkirk, Brest, Biarritz, Madrid, Strassburg, Nantes, Talmay, &c. In each of these memoirs M. Villarceau has arranged the details in a very systematic manner, so that no difficulty is found in following the course of the whole operation from the beginning to the end. In performing this work, for which he was so well fitted, he had evidently relieved M. Le Verrier of much labour which would ordinarily have devolved upon the Director. M. Villarceau was also intrusted, under M. Le Verrier's direction, with the preparation of the plans of the great Equatorial, erected in the west tower of the Paris Observatory.

The total eclipse of the Sun of July 18, 1860, was observed by M. Villarceau and other French astronomers at Moncayo, Spain, but the results of the observations were not published till 1868, when the detailed report, printed in the *Comptes Rendus*, was presented to the Académie des Sciences.

In 1857 M. Villarceau was appointed a member of the Bureau des Longitudes, and in 1867 a member of the Académie des

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Sciences in the section of Geography and Navigation. He was an Officer of the Legion of Honour, and Chevalier of the Order of Charles III. of Spain, &c. He was elected an Associate of the Royal Astronomical Society on June 10, 1853.

During the important period when the reorganisation of the Paris Observatory became necessary after the death of M. Arago, in October 1853, M. Villarceau afforded great assistance to M. Le Verrier. During a fortnight's residence at the Observatory in 1854, the writer of this notice had many opportunities of observing the personal interest that he took in the ordinary work, which then had hardly settled down to the regular routine duties which M. Le Verrier was so anxious to introduce. But it did not require many days to perceive that M. Villarceau was more at home when employed on mathematical investigation than in active work at the telescope. But perhaps this was owing partly to the physical defect of deafness, to which he was even then subject. This infirmity increased in after years, and was the means of isolating him, in some measure, from the pleasure of association with his scientific colleagues.

No one would have considered from the appearance of M. Villarceau that his end was so near. Little more than two months before his death he was selected by the French Government, with Colonel Perrier and M. Faye, to represent them as delegates to the International Geodetic Congress held at Rome in October 1883. He afterwards visited Naples, and, notwithstanding his age, he ascended Vesuvius, and was taken to the edge of the crater. It is possible that the fatigues incidental to this Italian journey may have so weakened his constitution that his strength was not sufficient to overcome the severe illness which seized him about the middle of December, from the effects of which he succumbed after only a few days' suffering. He died on December 23, 1883, at his residence in the Boulevard Saint Michel, Paris, having nearly completed the seventy-first year of his age.

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Proceedings of Observatories.

The following Reports of the proceedings of Observatories during the past year have been received by the Council from the Directors of the several Observatories.

Royal Observatory, Greenwich.

The usual meridian observations of the Sun, Moon, planets, and fundamental stars, with other stars from a selected list, have been continued with the same regularity as in past years. A new working catalogue of 2600 stars, comprising all stars down to the sixth magnitude, inclusive, which had not been observed with the Transit Circle since 1860, was brought into use last spring, and considerable progress has been made in clearing off the stars, three observations being usually required of each. About 1550 stars were observed in 1883, and special pains were taken to secure at least two observations of each of these in the year, so that mistakes of observation or reduction might be more readily detected. The whole number of transits observed during 1883 is 5318, and of circle observations 5180. Although the number of observations last year is much larger than in former years, the reductions have been kept up as closely as usual, and good progress has been made in collecting the planetary and star results for the year in the usual ledgers.

The mean error of the Moon's tabular R.A., from 100 observations with the Transit Circle in 1883, is $+0^s.03$, Prof. Newcomb's corrections to Hansen's *Tables de la Lune* having been applied in forming the tabular places of the *Nautical Almanac*. The mean error in 1882, when Hansen's tabular places, uncorrected, were used, was $+0^s.82$.

The new mounting of the collimators, described in the last Report, has been found to answer well. The variations in the micrometer readings from day to day are small, and the changes during the observations of collimation are practically insensible; but to avoid any possible effect from this cause it is still the rule to place the corresponding wires of the two collimators in coincidence immediately after as well as before each determination of the collimation-error of the Transit Circle.

Five determinations of the flexure of the Transit Circle were made in 1883. The mean resulting value, $-0''.49$, is larger than usual. The discordance between the nadir observation and the mean of the results from reflexion-stars, north and south of the

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zenith, is 0″·45 for 1883, a slight increase on the value found for the previous year.

The observations of the Moon and adjustment stars with the Altazimuth have been confined during the past year to the first and last quarter of each lunation, as it is considered that the intervening semi-lunation is sufficiently provided for by the meridian observations.

A gun-metal focussing rod, with scale reading to 1-200th of an inch, and ivory point for contact with the surface of the mercury, has been applied to the reflex-zenith-tube, and about 50 transits of γ *Draconis* have been taken during the past year for determination of the values of 1 revolution of the micrometer screw, corresponding to different temperatures and different scale-readings of the focal length, in order to obtain the correction for effect of temperature on the results with the reflex-zenith-tube.

The Equatorials have been employed for the usual extra-meridian observations. Comet *a* 1883 was observed on four days with the Sheepshanks Equatorial; and Comet *b* 1883 (Pons-Brooks) on seven days with the Sheepshanks Equatorial, and on two days with the S.E. Equatorial. Micrometric measures of the positions of five of the satellites of *Saturn*—*Tethys*, *Dione*, *Rhea*, *Titan*, and *Japetus*—have been made on six nights with the S.E. Equatorial, and of *Titan* only with the Sheepshanks Equatorial on seven nights, this telescope not being sufficiently powerful for satisfactory observation of the fainter satellites. Measures of one of the satellites of *Uranus* (*Titania*) have been obtained on one night with the S.E. Equatorial, and also some comparisons of *Polyhymnia* with neighbouring stars. Some measures of the distance and position-angle of double stars have been made on fourteen nights with the double-image micrometer mounted on the Sheepshanks or Simms Equatorial.

Nine occultations of stars by the Moon, and 37 phenomena by *Jupiter's* satellites, have been observed in 1883.

The usual spectroscopic observations have been made with the half-prism spectroscope mounted on the S.E. Equatorial. The displacement of the F, *b*, or D lines has been measured in the spectra of 46 stars, the total number of measures being 309. The chromosphere of the Sun has been examined on 25 days, several prominences being visible on each day, and the spectra of various Sun-spots on eight days. The spectrum of Comet *a* 1883 was examined on one night, and that of Comet *b* 1883 (Pons-Brooks) on two nights. The results of these observations have been given in the *Monthly Notices*. The spectrum of η *Ceti* has also been specially examined at the request of Dr. Konkoly, but the supposed existence of bright lines has not been confirmed. The reductions of all the spectroscopic observations are completed to the end of 1883.

Photographs of the Sun have been taken on 221 days in 1883, as compared with 201 in 1882, an increase which is due partly to

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finer weather, and partly to the use of dry plates instead of wet. the former having been used regularly since December 1882, Sun-spots have not shown any sensible decrease as regards number or area in the year 1883, though no single spot as large as that of 1882, November 12–25, has been observed during the year. The faculæ, however, show a decided falling-off in 1883 as compared with 1882. The Greenwich photographs have all been measured in duplicate to the end of 1883, and completely reduced to November 17. Seventy-eight Indian photographs, taken at Dehra-Dûn under the superintendence of Mr. J. B. N. Hennessey, have been received from the Solar Physics Committee, to fill up the gaps in the series of Greenwich photographs between January 1 and October 19. All of these have been measured, and the measures completely reduced to September 6. One hundred and forty-two Indian photographs for the year 1882 have been measured and completely reduced, thus making the total number of 343 days for which there are photographs of the Sun available in 1882.

In the spring of 1883 an important addition was made to the instruments of the Observatory by the munificent donation of the Misses Lassell, who have presented the 2-feet Equatorial reflector which had done such good service in the hands of their late father, W. Lassell, Esq. The instrument has been erected in the South Ground, where it commands a nearly unobstructed view of the sky to within 5° of the horizon. A circular building 30 feet in diameter, to be surmounted by a dome, has been prepared to receive it. The dome, which has been constructed by Messrs. T. Cooke and Sons, is now in process of erection.

The volume of *Greenwich Observations* for 1881 was distributed in June last, and the printing of the astronomical portion of the volume for 1882 is complete with the exception of the Introduction. The Spectroscopic and Photographic Results for 1882, which include the results from the Indian photographs, are also printed. The printing of the Magnetical and Meteorological results has been a little delayed by some additions, which have been made with a view of giving fuller information as to magnetic disturbances whether of large or small amount, and of thus affording facilities for comparing them with solar phenomena.

Armagh Observatory.

During the past year very little observing has been done, most of the time having been taken up by the preparation for the press of the *Second Armagh Catalogue of* 3000 *Stars*. It is expected that the printing will commence in a few weeks.

In June the Governors of the Observatory sent a memorial to the Treasury (accompanied by a printed historical account of the institution), asking the Government to grant compensation for

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the pecuniary losses to the Observatory caused by the passing of the Irish Church Act. In December an answer was returned, stating that the Lords of the Treasury had decided to insert in the estimates for the coming year the sum of 2,000*l.*, to be spent by the Governors solely on instruments or apparatus. It may thus be hoped that the instrumental equipment of the Observatory will soon be materially improved.

The working of the photographic Barograph and Thermograph was stopped at the end of the year, in consequence of the withdrawal of the greater part of the grant hitherto paid by the Meteorological Council. The self-recording rain-gauge and Anemograph remain in operation, and 9 A.M. and 9 P.M. readings are now taken as at second-class stations.

Cambridge Observatory.

A steady effort has been made throughout the past year to obtain observations of such stars in the Zone which has been assigned to this Observatory, as had been hitherto unavoidably omitted. All other work has been treated as subordinate to this. As the stars to be observed were irregularly scattered over the Zone, the actual number of observations obtained is considerably less than in former years, although the time devoted to them has been nearly the same.

The total number of meridian observations is 2289, including 248 observations of Clock Stars, 69 observations of *Polaris* above the pole, requiring 164 Circle Readings, and 70 below the pole, with 166 Circle Readings, and 1902 observations of Zone stars.

The fluctuations in the position and adjustments of the Transit Circle have kept within narrow limits, but have been carefully watched. The error of Collimation was determined 180 times, and the level error and Nadir Point were each determined 169 times.

Swift's Comet was observed on 9 nights with the Northumberland Equatoreal.

In addition to the above-mentioned work we have nearly completed a very extensive series of observations made for the purpose of testing the accuracy of the divisions in the West Circle, which is the one that has been used throughout in the Zone observations. The observations of the Standard Stars are reduced to the end of 1881, both in Right Ascension and North Polar Distance. The observations of 1882 and 1883 are in hand, and the reductions are far advanced. The true Right Ascensions and true North Polar Distances of the Zone stars are obtained to the end of 1881, and the corrections for reduction from apparent to mean places to the end of 1880.

Tables have been constructed for reducing to the epoch

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1875·0, and by the aid of these the work of reduction is progressing rapidly.

The small corrections for reducing to the Berlin places of the standard stars, and for taking into account the flexure of the instrument, are calculated nearly up to the present time.

Dunsink Observatory.

On 118 nights during the year the chronograph was used in connection with the meridian circle. On only 52 nights, however, was it sufficiently clear to observe the Southern stars on the working list, varying in magnitude from about 6·5 to 10; of these, 437 satisfactory observations in Right Ascension were to be taken, and in Declination 474, all of which have been reduced to their apparent places for the date of observation. The reduction to mean places has just been commenced. Both coordinates of each star were observed at each transit, but on one or two occasions the contact in the clock or at the trigger was unsatisfactory, and in consequence the chronograph sheet illegible, which accounts for the difference between the numbers just given.

Observations of 280 standard stars were taken for determining the Right Ascensions, the zero of Declinations being on every occasion determined by Nadir observations from a mercury trough. In addition, observations of standard stars were taken on 66 nights when the weather was too bad for regular observations to determine the clock error for the time service to Dublin.

The South Equatorial has been employed as usual in observations for Annual Parallax; and Part V. of the *Dunsink Observations* is going through the press.

Royal Observatory, Edinburgh.

During the past year the daily electric distribution of true time, by ball, gun, and controlled clocks, as derived from astronomical observation, has been kept up as usual. Also the computation of the bi-diurnal meteorological observations of six towns, and fifty-five town and country stations throughout Scotland, for the monthly and quarterly returns of the Registrar-General of Births, Deaths, &c., in Scotland.

It was hoped by the Director that during the year some action would be taken with respect to the recommendations of the Board of Visitors in 1870 and 1872, and the Commissions of Enquiry in 1876 and 1879; but this hope has been disappointed. Correspondence is in progress with respect to the resumption of the printing of the Edinburgh Star Catalogue in a condensed and economical form.

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On April 6 the Universities (Scotland) Bill was introduced into Parliament, by which it was proposed that the Observatory be attached to the University of Edinburgh; but it was withdrawn.

Kew Observatory.

The sketches of Sun-spots as seen projected on the photo-heliograph screen have been made on 214 days, in order to continue Schwabe's enumeration. The Sun's surface was found to be free from spots on seven of those days.

The correction to the area-measurements for foreshortening, which, at the date of the last Report, had not been applied to the reduction of Sun-spot observations for the last two years of the series, has since been made under Mr. De La Rue's direction. The whole series is at the Royal Society, and is now being revised and arranged for reference by Mr. Marth, on behalf of the Council of the Royal Society, who made a grant of money for that purpose.

One hundred and fourteen observations have been made of Sun-transits, for the purpose of obtaining correct local time at the Observatory; 224 clock and chronometer comparisons have also been made. Shelton's Clock, K.O., has been used as the standard timepiece of the Observatory.

The Committee, having decided to make a trial of a system of watch-rating for the public, have granted £100 for the preliminary expenses. In accordance with a scheme prepared by the Superintendent, and with a view to the obtaining of star-transits, they have built a temporary transit-house in a more convenient locality than the present one, in which the Sheepshanks 30-inch Transit (No. 27), lent by the Royal Astronomical Society, has been set up. An apparatus is being fitted up for rating the watches at extreme temperatures, and a circular has been drafted, which will be issued to watchmakers on the completion of these arrangements. It is hoped that operations will be commenced very shortly.

Prof. C. S. Peirce, of the United States Coast Survey, who made a series of pendulum observations at Kew and elsewhere in 1876, visited the Observatory in July last, and made a subsidiary series of experiments with a view of determining the flexure of his stand when on the Kew piers, using a noddy for the purpose.

The usual magnetical and meteorological observations and reductions, and the verification of instruments, operations to which the attention of the Observatory is mainly directed, have been carried on as formerly.

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Liverpool Observatory, Bidston, Birkenhead.

The Meteorological work during the past, and for several preceding years, has been very heavy for the limited force of this Observatory, but this has not prevented due attention being paid to work connected with improvements in chronometric navigation. In addition to the testing of new chronometers for error of thermal adjustment, records of the performance of chronometers at sea for each of one hundred voyages from Liverpool to the West Coast of South America and back to Liverpool have been received and discussed at this Observatory. The rates of all the chronometers employed were corrected for change of temperature in accordance with the mean temperature obtained daily at sea. As each ship carried three chronometers there are three hundred cases in which the daily rates at sea were obtained for an average voyage of one hundred and five days. The average difference between the predicted daily rates and the rates actually made at sea was 0·37 of a second of time. In one hundred out of the three hundred cases the average difference between the predicted rates and the rates made at sea was only 0·08 of a second of time. The thermal errors of all the chronometers used were in the first instance carefully determined at Bidston.

A Report on the working of the chronometrical department of this Observatory has recently been published by the order of the Mersey Docks and Harbour Board, which shows the method had recourse to for predicting the rates on the voyages above named. The results show that when the rates are corrected for change of temperature, marine chronometers will carry on the Greenwich time at sea for three or four months as accurately as ordinary astronomical clocks on shore will carry on the time for a similar period. But when the rates are not corrected for change of temperature, the errors of chronometers at sea are, on the average, increased in the proportion of about four to one.

Radcliffe Observatory, Oxford.

Observations have been systematically made with the Transit Circle during the year 1883.

The number of observations is as follows:—

Transits	2604
Circle Observations (each requiring the reading of the four Microscopes)...	2447

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These totals include :—

Observations of the Sun	54
Observations of the Moon	53
Reflexion Observation of Stars	96

and

234 Pairs of Nadir Observations

Only one observation was made of the passage of the Moon's diameter, but ten measures of the vertical diameter have been secured.

The N.P.D. reductions are finished to date, and the R.A.'s are completely reduced to September 5.

The Astronomical Results of 1881 have been printed.

The Observations for 1882 are being prepared for press.

The Nadir Points have been exclusively determined with the Nadir reflecting eyepiece; but frequent observations of stars by reflexion, North and South of the Zenith, have been made as a check upon the existence of any systematic errors.

The value of R—D, as determined from 26 Northern stars with 56 observations, is

$$-0''.094;$$

whilst from 29 Southern stars with 57 observations, its value is

$$-0''.284.$$

The mean discordance between the Nadir Points determined from the wire observations and from the star is therefore only

$$-''.09.$$

As another check upon the proper working of the instrument, determinations of the Nadir reading have been taken over a considerable range of the screws, and the results show that there are no serious errors in the runs of the screws.

Some R.A.'s have also been observed by reflexion to test the freedom of the instrument from systematic errors, and as far as these observations can be carried, they give no indication of such errors.

Eleven observations of Occultations of Stars by the Moon and forty-five of Phenomena of *Jupiter's* Satellites have been made with the extra-meridional instruments.

The Meteorological Results for 1881 are rapidly passing through the press. The complete volume will soon be ready for distribution.

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Oxford University Observatory.

Since the last report of the work done at the Oxford University Observatory, then brought up to Feb. 1883, the expedition to Abbasseeyeh, in the neighbourhood of Cairo, for the purposes of stellar photometry has been completed, and the results have been communicated to the Society. The more interesting of these are as follows:—the climate, as was well known before, is, during the winter and early spring months, equable and dependable beyond anything known in Europe. This was shown by the fact that 3,385 extinctions of stars were made at Cairo with the wedge photometer, while only 997 were made at Oxford in the same time. The absorption of stellar light by the atmosphere was equal to $0^m \cdot 187$ sect. Z.D., while at Oxford it is $0^m \cdot 253$ sect. Z.D., indicating, as might be supposed, a superiority of translucency in the Egyptian atmosphere. Still, when viewed in a practical sense, this superiority is not so important as it appears at first sight, inasmuch as the minimum visibility of stars in Egypt differs from that in England, at an altitude of 30°, by only one-fifth of a magnitude. There is, however, one evident superiority in the Egyptian over the English atmosphere, that whereas it is not safe (ordinarily) to work photometrically at zenith distances greater than 60° in England, observations in Egypt may easily be continued to 75° Z.D. Since the Cairo results have been published, another determination of the absorptive power of the atmosphere has been effected by Dr. Müller, of Potsdam, with the result, that the formula $0^m \cdot 201$ sect. Z.D. fairly represents it. The general conformity of all these results, from Bouguer and Seidel to the present day, by means of entirely different instrumental appliances and methods, is gratifying and remarkable.

The photometric measures of all the stars brighter than the fifth magnitude, extending from the Pole to a little beyond the Equator, have been completed and published by the Society in vol. xlvii. of their *Memoirs*. In addition to the photometric determination, a concise account of the methods employed for effecting the same purpose by successive astronomers from the earliest attempts to the present day has been added.

The photometric measures of all the remaining stars, within the limits described, contained in Argelander's *Uranometria*, are now being proceeded with. Considerable advance has been already made, and it is hoped that at the present rate of progress a new Uranometria will be completed in fifteen months from the present date. This will constitute the first complete attempt of the kind in our country.

Besides this continuous photometric work the measurement of the relative coordinates of forty stars of the *Pleiades* has been completed, with the duplex micrometer (together with their photometric magnitude), with a view to the discussion of their proper

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motion, and the cohesion, or otherwise, of the group, as forming one system. This undertaking has involved considerable time, labour, and difficulty, owing to the necessity of examining so many catalogues of observations and of reducing the whole on one system, in accordance with the requirements of modern refinement. Probably, the memoir will be ready for presentation to the Society before it separates in June. There are necessarily considerable interruptions to these researches owing to the primary duties of the Director of the Observatory in lecturing as a University Professor.

All desirable aid is received from the assistants, Mr. W. E. Plummer and Mr. Jenkins.

Temple Observatory, Rugby.

During the year 1883, the measurement of positions and distance of double stars has been continued, 263 sets of measures of 109 double stars have been made by Messrs. Seabroke and Percy-Smith, with the Alvan Clark Refractor. The results of the last three years have been reduced, and are being prepared for printing.

Owing to an irregularity noticed in the rate of two clocks governed by pendulums having wooden rods, compensated by zinc tubes, of a length based on Captain Kater's figures for the coefficient of expansion of wood, a series of experiments on the expansion of rods of various woods was undertaken by Mr. Seabroke, with the result that the numbers usually given appear much too high.

The usual instruction has been given to members of the School, to whom the Observatory has been open for an hour every fine evening.

Stonyhurst College Observatory.

The Sun observations have now become one of the chief portions of the daily routine work of this Observatory, and results have been obtained on 202 different days. The large pictures of the whole solar surface are 185 in number, and these have been supplemented by 16 smaller pictures, and by general descriptive notes when drawings could not be obtained. It is hoped that the series of solar drawings, now extending over several years, will be measured in the course of a few months, and some results laid before the Society.

The height of every part of the chromosphere has been measured on 72 days, and a certain portion on 7 other days. The observations of spot spectra were much less numerous than those of the chromosphere; but more attention will be given to this

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important branch of solar physics during the ensuing year. The unusual amount of cloudy weather has greatly interfered with night work, and only preliminary observations have been attempted with the large star spectroscope. Positions of Comet Pons were taken on October 6, 25, and 28, and on November 26. The phenomena of *Jupiter's* satellites were observed on 33 occasions, and also the occultation of *o Arietis*, on March 12.

The meteorological and magnetic departments have continued to receive the same attention as in previous years, and the continuous photographic record of the principal instruments has suffered no interruption.

Mr. Barclay's Observatory, Leyton, Essex.

The past year has been exceedingly unfavourable for Astronomical work in the Observatory, a long continuance of cloudy nights having been the rule, though the rainfall has been small, and the first four months remarkably dry.

The usual routine work has been carried on, and observations of the satellites of *Saturn* have been made when possible, Mr. Marth's Ephemerides being used.

Mr. Common's Observatory, Ealing.

During the past year attention has been chiefly given to photography. Negatives of several nebulæ have been obtained, including a series of the Great Nebula in *Orion*, using the arrangement for making long exposures completed in 1882. A 6-in. Equatorial Refractor has been mounted specially for stellar photography, and a number of different lenses tried.

Another mirror has been figured by Mr. Calver for the 3-foot telescope.

The weather during the last half of the year has been very unsuitable, and very little work has in consequence been done.

The Earl of Crawford's Observatory, Dun Echt.

Owing to the serious illness of Mr. Lohse, and the continued absence of Dr. Copeland in South America, but few observations were made in the earlier part of the past year. Still Comet 1883 I. was not allowed to pass away without careful spectroscopic observations having been made of the usual three bands on March 1. These are published in *Copernicus*, along with a few places of the returned Comet of 1812.

During the year fifteen circulars, all referring to comets, were printed at the Observatory, and widely distributed. Dr.

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Swift has since found that the object mentioned in Circular 81 is in reality a nebula, the appearance of motion which led to a subsequent mistaken identification having arisen from a defect in his apparatus which he has since remedied.

The meteorological and time observations, as well as the weekly firing of the time-gun on Saturday, remain unaltered.

In August the optical resources of the Observatory received a most important augmentation through the arrival of a large grating, ruled on speculum metal, under the supervision of Professor H. A. Rowland, of Baltimore. This magnificent grating has a ruled surface of $5\frac{1}{16}$ by $3\frac{1}{2}$ inches, at the rate of 14,438 lines to the inch. With collimating and viewing telescopes of four inches aperture, a power of 120 can be advantageously used on the spectra of the second order. In this way the duplicity of D_2 was noticed, and that of D_1 suspected, independently of Professor Rowland's separation of both those lines into two components. On December 14 it was observed that the strong line wave-length 588·32, although double on the Sun's following limb, is but single on the preceding limb; the one component being due to the iron of the Sun's atmosphere; and the other, most probably, to aqueous vapour in our own, the displacement of the former line being, of course, caused by the Sun's rotation.

Dr. Copeland returned on September 1, after having established a temporary observatory at Puno, 35 feet above the surface of Lake Titicaca, and 12,540 feet above the sea. Subsequently the telescope was also erected at Vincocaya, 14,360 feet, and for a single day only at Arequipa, 7,550 feet above the ocean. The then disturbed political state of Peru in some degree impeded the progress of the work and the transmission of essential parts of the instruments; and but for the cordial and energetic kindness of Mr. John L. Thorndike, the lessee of the Puno, Mollendo, and Cuzco Railway, the difficulties encountered would have been far greater. This remarkable railway, attaining a height of 14,666 feet, was made the base of operations, and the reversed head-stock of a lathe from one of its workshops formed the Equatorial mounting with which most of the observing was done. Several close double stars were detected, β *Muscæ* and H *Velorum* being amongst the number; but the definition, although often very fine, was not greatly superior to that at a good station lower down. The chief gain was in increased transparency of the air, a measure of which may be found in the visibility of *Sirius* and *Canopus* to the naked eye before sunset. An interesting result is the discovery of several minute planetary nebulæ, by the method of sweeping with a prism attached to the eye-piece recommended by Professor Pickering, and not less interesting is the finding of a number of small stars with bright-line spectra, closely related to those of the Wolf-Rayet stars in *Cygnus*, and the establishment of the fact that the marvellous spectrum of γ *Carinæ* (*Argûs*) noticed long ago by Respighi and Le Sueur is also of

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this class. These spectra gain in importance from their great resemblance to those of temporary stars. As a rule the air was very dry, the tension of aqueous vapour falling below $\frac{1}{10}$th of an inch. On one occasion the dry bulb thermometer stood at 45°·7, while the wet bulb showed 30°·6, and the solar-radiation thermometer registered 199°·1 Fahr., the boiling-point of the station (Vincocaya) being 186°·1. The Sun's corona was not certainly seen, although the strong diffused light was restricted to the immediate neighbourhood of the Sun's disk, where it very suddenly increased. This is confirmed by the observation of a spectrum with Fraunhofer lines terminating somewhat abruptly at about 5′ from the Sun's limb. A summary of these experiences was read before Section A of the British Association at Southport, and a complete Report, including all the results, is nearly ready for publication.

Mr. Edward Crossley's Observatory, Bermerside, Halifax.

The usual observations have been carried on at this Observatory throughout the year 1883. About 300 measures of binary stars were made, and about 40 of the phenomena of *Jupiter's* satellites were observed. Some of these latter observations have already appeared in the *Monthly Notices* of the Society. The usual meteorological observations have been made at 9 A.M. and 3 P.M. daily, and monthly summaries sent to the Registrar-General, &c.

A Wedge Photometer has recently been procured from Mr. Hilger, and we hope to be able to do some useful work on variable stars next year. The year 1883 proved a bad one in point of weather, the sky being clear throughout on not more than 20 nights.

Mr. Huggins' Observatory, Upper Tulse Hill, S.W.

The great Equatorial was remounted in the early part of the year. The improvements consist mainly in having had the declination made double: *i.e.* there are now two declination axes, the one moving within the other; to one is attached the refractor with an object-glass of 15 inches diameter, to the other the Cassegrain telescope, with a metallic speculum of 18 inches diameter. The double axis permits the telescopes to be moved independently of each other in declination, so that the one not in use may be so moved as not to come into contact with any part of the mounting, or to be in inconvenient positions.

Both telescopes are carried by the clock motion, and by the doing away with the counterpoise weight, the weight to be carried is not increased in a serious degree.

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The photography of the Corona without an eclipse has formed the principal work since the remounting of the Equatorial. A new apparatus has been constructed which is attached to the refractor, so as to be carried by the clock motion and under the same control as the telescope itself. This consists of a 7-foot Newtonian telescope made by the late Mr. Lassell, and kindly placed in my hands by the Misses Lassell. The speculum, $7\frac{1}{4}$ inches in diameter, possesses great perfection of figure and still retains its original fine polish. The small mirror is removed and the open end of the tube fitted with a mahogany cover. In this cover, on one side is a circular hole $3\frac{1}{4}$ inches in diameter for the light to enter; below, is a similar hole, over which is fitted a framework to receive the "backs" to hold the photographic plates, and also a frame with fine ground glass for putting the apparatus into position. Immediately below, towards the speculum and very near the focal plane, is fixed a shutter with an opening of adjustable width, which can be made to pass across more or less rapidly by the use of indiarubber bands of different degrees of strength. In front of the opening through which the light enters is fixed a tube about 6 feet long, fitted with a large number of diaphragms. It is obvious that if the apparatus be so directed that the light passing through the upper opening falls upon the middle part of the speculum, which alone comes into use, the image of the Sun will be formed in the middle of the lower opening, about 2 inches from the position it would occupy if the tube were directed axially to the Sun.

Cells containing coloured fluids have been discarded in consequence of the rapid decomposition of the fluids under the Sun's light, and the formation of a fine deposit on the surfaces of the plates which would scatter light, and might also give rise to misleading appearances in the photographs. The selective power of the coloured cells was sought to be obtained in the sensitive film itself, and it was found that the silver chloride emulsion was very sensitive for this purpose. Disks of brass were made of such a size as to be in a minute degree larger than the Sun's image.

About fifty photographs were taken between April 2 and Sept. 4. Some were taken with the Sun covered with the brass disks, some without. In some cases the Sun was in the centre of the field; in other cases several photographs were taken with the Sun towards different sides of the field. All these plates show a more or less coronal appearance about the Sun. The plates are now in Mr. Wesley's hands, who is making drawings from them. In three plates taken about the period of the eclipse, a strong rift is seen which is similar to the main feature in the photographs taken at Caroline Island during the eclipse of May 6.

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The Earl of Rosse's Observatory, Birr Castle.

Astronomical observations were made on 64 nights.

In the earlier part of the year twenty-four complete drawings of the planet *Jupiter* were made, besides a few sketches of parts of the planet's surface. The 3-foot Reflector was the instrument employed.

Nebulæ were observed, and a few sketches made, early in the year with the 6-foot Reflector under very unfavourable circumstances, owing to the condition of the speculum at that time.

In August a new machine for polishing 6-foot specula was completed. It is similar in construction (except as to the mode of tilting the speculum for testing) to that constructed for the 3-foot specula in the previous year. The speculum was polished with it successfully so far as it was tested, but the weather has been so exceedingly unfavourable during the latter part of the year that it was not possible to try it on test objects so fully as was wished.

The addition to the driving-clock of the 3-foot telescope of an escapement under the electric control of a clock in the Observatory, affords great assistance and great comfort in various classes of observations with that instrument.

Some photographs of stars have been taken with various exposures, and two or three unsuccessful attempts to obtain an image of the Great Nebula in *Orion* were made. Owing to the greater focal length of the 3-foot Reflector as compared with that of Mr. Common, three times the exposure that was required to produce his well-known plate was in one case given, but from the appearance of the stars in the negative it would seem that, whether owing to greater intensity of light in the primary image, or to a lesser sensitiveness, or a difference of treatment of the dry plate, the result was not equivalent to that of his or the late Prof. Draper's photograph.

Of late an unusually cloudy sky, accompanied by unusually poor definition has impeded us in all our observations. It is somewhat remarkable that not a single sketch of the planet *Jupiter* has been made since its reappearance last autumn, and this is at least as much due to unusually bad definition as to the interference of photographic and other work.

The historical and critical discussion of the literature "On the Influence of Magnetism on the Rate of a Chronometer," by Dr. Boeddicker, alluded to in our last Report, has since been published in the *Scientific Transactions* of the Royal Dublin Society.

The meteorological department has been carried on, as in the previous year, without change.

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Colonel Tomline's Observatory.

The period over which the following report extends is from January 1882 to December 1883, with the exception of four months, during which time the observer was engaged in the work of observing the Transit of *Venus*, at Bermuda. The Equatorial has throughout been entirely devoted to the observation of comets, four of which have been followed in the interval, namely:—

Comet I. 1882	From Mar. 24 to June 3	42 evenings
„ III. 1882	„ Sept. 22	1 „
„ I. 1883	„ Mar. 3 to Apr. 9	11 „
„ Pons 1812	„ Sept. 8 to Dec. 23	24 „

the last being still under observation.

The reduction of the later observations are in an advanced state, but their publication is delayed principally by the difficulty of obtaining satisfactory places of comparison stars. It is found that fully 22 per cent. of the stars selected for comparison with comets are not previously observed on the meridian; and in accordance with a plan always adhered to, no observations are published until the whole of these have been identified, and micrometrically compared with some neighbouring known star.

In spite of these drawbacks some progress has been made in bringing up arrears, the following observations of comets having been published in the *Astronomische Nachrichten* during the period under consideration.

Comet II. 1879	6 observations,	made between	June 26	and July 24
„ IV. 1879	3 „	„	Sept. 8	„ Sept. 15
„ V. 1879	13 „	„	Sept. 10	„ Oct. 15
„ II. 1880	13 „	„	Apr. 30	„ Sept. 9
„ III. 1880	11 „	„	Oct. 18	„ Nov. 25
„ V. 1880	7 „	„	Dec. 21	„ Jan. 31
„ III. 1881	45 „	„	June 24	„ Nov. 17
„ IV. 1881	13 „	„	July 27	„ Aug. 28
„ I. 1882	42 „	„	Mar. 24	„ June 3

In future our efforts to maintain the reductions to date will be continued, it is hoped with greater success.

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Royal Observatory, Cape of Good Hope.

During the past year the work of the Observatory has been confined to the completion of the programme announced in the *Monthly Notices* for March, 1883.

H. M. Astronomer has reached England on leave of absence, and brings with him for publication the following works in MSS.:—

1. A Catalogue of 4810 Stars reduced to the Equinox 1850·0 from observations made under the superintendence of Sir T. Maclear in the years 1849, 1850, 1851, and 1852. The work of reduction was begun under the direction of Mr. Stone, and has been carried to completion by Mr. Gill.

2. 512 Occultations of Stars by the Moon observed between the years 1834 and 1881 at the Royal Observatory, Cape of Good Hope; reduced, and compared with Hansen's Tables.

3. An account of the Telegraphic Longitude operations connecting Aden and the Cape.

4. Results of Meridian Observations for 1879, 1880, and 1881.

5. Results of Extra-Meridian Observations for 1881, 1882, and 1883.

The series of observations undertaken with Mr. Gill's Heliometer has been brought to a close. The results have been presented to the Society, and will appear in the next volume of the *Memoirs*. This memoir contains investigations on the parallax of the following stars:

α Centauri, independently investigated by Gill and Elkin, each observer employing two pairs of comparison stars (four pairs in all).

Sirius, both by Gill and Elkin, each observer employing different comparison stars.

ε Indi, by Gill and Elkin, each observer employing different comparison stars.

Gill investigates also the parallax of *β Centauri*, Lacaille 9352, and o_2 *Eridani*.

Elkin investigates the parallax of *ζ Toucani*, *e Eridani*, and *Canopus*.

The observations of the Great Comet of 1882 are reduced, and a memoir on the subject is nearly ready for press.

The observations of *Victoria* and *Sappho* are still unreduced. The pressure of work in clearing off arrears of publication has made it impossible to undertake the reductions in question—besides which, all of the series of observations that have been made have not yet been received. Mr. Gill will undertake this work so soon as he returns to the Cape.

The meridian observations of 331 standard stars for the future Zone observations of Professor Schönfeld's *Durchmusterung*, and of specially selected stars observed for refraction in conjunction

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with Prof. Bakhuyzen at Leiden, have been completed. The meridian observations of the stars used in the longitude operations, of stars observed with comets, of comparison stars in the Heliometer parallax determinations and of other miscellaneous stars of the working list have also been completed.

The Transit Circle Observing List, begun 1884, Feb. 1, contains all stars in the southern hemisphere to fourth magnitude inclusive, all stars of the *Nautical Almanac*, and of the Berlin, Paris, and American Ephemerides to N.P.D. 45°; it also provides for daily observation of the Sun, *Venus*, and *Mercury*.

The new Collimators, which permit observation by reflection to 70° Zenith Distance, have been mounted. All stars will now be observed direct and reflex on alternate days.

The Geodetic Survey of South Africa which has been urged upon the Governments of the Cape and Natal by H.M. Astronomer since 1879, has at last been fairly begun. Captain Morris, R.E., with Lieut. Laffan and fourteen Sappers, arrived at the Cape in June 1883. The first base line has been measured in Natal, and the bars have been compared with the Observatory standard. A short account of these operations will be communicated to the Society.

The great 3-foot Theodolite lent by the Indian Government to H.M. Astronomer, has been sent to England for some alterations which will be made under the superintendence of Mr. Gill. The instrument, after these alterations, will be admirably adapted for the investigations on refraction to which it will be devoted.

Tide gauges at Table Bay, Port Elizabeth, East London, Port Alfred, and Durban have been established on the recommendation of Mr. Gill. The direction of the tidal observations at Durban is transferred to Mr. Neison, Government Astronomer of Natal; that of the other Stations remains under the direction of Mr. Gill.

The system of Time Signals has been further extended and improved.

The meteorological observations made in the year 1882 at the Observatory, together with those made in different parts of the colony, have been printed in the Reports of the Cape Meteorological Commission.

Dr. Elkin, who resided at the Observatory from the beginning of 1881 till the middle of 1883 as Mr. Gill's guest and friend, has left the Cape and accepted the charge of the new Heliometer at Yale College, New Haven, U.S. His work at the Cape has been from first to last a labour of love.

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Notes on some Points connected with the Progress of Astronomy during the past Year.

Discovery of Minor Planets.

The following four minor planets were discovered in the year 1883—two by M. Palisa, and one each by M. Borrelly and Dr. C. H. F. Peters:—

No.	Name of Planet.	Date of Discovery. 1883.	Discoverer.	Place of Discovery.
(232)	Russia	Jan. 31	Palisa	Vienna
(233)	Asterope	May 11	Borrelly	Marseilles
(234)	Barbara	Aug. 12	Peters	Clinton, U.S.
(235)	Carolina	Nov. 28	Palisa	Vienna

Ten planets discovered in preceding years have been recently named—

(220) Stephania	(224) Oceana	(228) Agathe
(221) Eos	(225) Henrietta	(229) Adelinda
(222) Lucia	(226) Weringia	(231) Vindobona
(223) Rosa		

The Supplement to the *Berliner Jahrbuch* for 1886 contains 26 daily ephemerides of the minor planets for 1884, the R.A. and Declination being given for Berlin mean midnight. These are fewer than usual, but many more will doubtless be issued in the *Jahrbuch* Circulars later in the year. Much attention appears to have been given to the calculation of the 20-day ephemerides, of which there are no less than 231, those omitted being for *Hersilia* and the four lately-discovered planets. There do not appear to be many cases of near approach to the Earth, four planets only coming within the mean distance of the Sun. These are *Æthra*, whose least distance near opposition is about 0·87; *Medusa*, 0·90; *Asia*, 0·97; and *Elsa*, 0·98.

The Comets of 1883.

Three comets have been observed during the year, of which one is the great comet of September 1882, already mentioned in the last Annual Report.

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The continued observation of this comet has been principally confined to the southern hemisphere; and notwithstanding the gradually diminishing brilliancy, this comet was seen, at Cordoba, up to June 1. At this date the distance of the comet from the Sun was 4·5 times the Earth's distance, and the true anomaly was about 175°. The determination of the orbit that will represent the path of this comet, through this unusually long period, with the accuracy usually sought in modern investigations has not yet been effected, and consequently the mean motion is still in some measure doubtful, though there can be little doubt but that it corresponds to a period of between 700 and 800 years. Inclined at an angle of 38° to the elliptic, the comet must soon pass out of the range of planetary perturbation, and therefore the observations should furnish an example of pure and undisturbed elliptic motion; but, as intimated in the last Report, the shifting form of the nucleus, and the different points selected for observation by different observers, have introduced unusual difficulties in the formation of normal places, and therefore the application of the usual method of equations of condition cannot here be successfully applied. Dr. Morrison, whose calculations on this subject are perhaps the most thorough yet published, has preferred to rest his calculations on three selected positions between September and November, when the character of the nucleus was tolerably well defined, and to draw an ellipse through these. By this means he has succeeded in representing the motion of the comet, from the date of its discovery up to the beginning of December, with the utmost accuracy, and finds that the observations both before and after perihelion can be represented by a continuous curve. With the later observations, however, residuals of very considerable amount occur, and exceed the amount of error that it is suggested is introduced by the varying form of the nucleus. The amount of error occasioned by this circumstance is placed by Dr. Morrison at 30″ in R.A., and much less in declination, but errors of double this amount occur between the computed places and the observations which he has selected. Whether the residuals can be reduced or not, it is certain that the alteration in the elements will be very trifling, and consequently the period of 712 years assigned by the calculation will be but little changed. With this period there is some probability that this comet is another apparition of one that is recorded in the catalogues as having been seen in B.C. 370, and possibly again in A.D. 1131 or 1132.

On Feb. 23, shortly after the rising of a nearly full Moon, Mr. W. R. Brooks, of Phelps, N.Y., detected a telescopic comet not far from *β Pegasi*. The motion, nearly due east, carried the comet through *Andromeda* into *Aries* and *Taurus*, and therefore the comet always remained near the evening twilight. Moreover, the comet had already passed its perihelion passage, and as the distance from the Earth was also increasing, it remained an inconspicuous object. Observations were continued at several

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Observatories up to early in April, but as the orbit does not resemble any previously computed, and does not show any signs of eccentricity, but little interest attaches to this comet. The spectrum was of the ordinary three-band type.

On Sept. 1 Mr. Brooks discovered a second comet, of extreme faintness, and situated in the constellation *Draco*. When it had been sufficiently observed, and an approximate parabolic orbit derived, it was at once evident that this object was a second apparition of the comet of 1812. The result of the discussion of the original observations of Pons' Comet by MM. Schulhof and Bossert was to place the approaching perihelion passage in the following September, or about 220 days later than it actually occurred. A closer agreement could scarcely have been expected, seeing that in 1812 the observations, extending over barely two months, did not suffice for an accurate determination of the mean motion and eccentricity. MM. Schulhof and Bossert have shown, however, that by an alteration of only −0·0002700 in the eccentricity, their resulting elements would have been sufficiently close to represent the observations with a fair amount of accuracy. Making this alteration, the deduced elements from 1812, and those derived from observations up to Jan. 9, are as follows (the altered eccentricity being retained, since the observations hitherto made are not sufficient to correct it) :—

	Orbit from 1812 Observations.		Orbit from 1883 Observations.	
T	...		1884, Jan. 25, 7546, Berlin	
$\pi-\Omega$	199° 12′ 32″·5		199° 11′ 33″·2	
Ω	254 6 15·3	Eq. 1880.	254 5 42·3	Eq. 1880.
i	74 3 20·4		74 2 35·7	
e	0·9549960		0·9549960	
log. q.	9·8893650		9·8897099	

The recognition of the identity of this comet with that of Pons' might have been effected earlier had M. Schulhof begun the sweeping ephemeris at a greater angle from perihelion. This ephemeris commenced at a true anomaly of −97° 30′, and the comet was actually found at an anomaly of −113°. The comet has now passed into the southern hemisphere, where it will probably be visible till next June.

Although the brilliancy of the comet has increased till it has become a fairly conspicuous object in the south-western sky, this increase has not been effected steadily, and in accordance with the theoretical law. M. Bigourdan has stated that between Sept. 5 and 24 the increase in the brilliancy of the comet was thirty or forty times greater than would have resulted from reflected light alone. Mr. Chandler, of Harvard, from a comparison of his own observations with those of Prof. Schiaparelli, has, however, decided the time of the outburst within much nar-

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rower limits, and fixes it between Sept. 22, $7^h\ 45^m$ and $11^h\ 45^m$ G.M.T. Another outburst has been actually watched by Dr. Müller, of Potsdam, and rigorously recorded by observations with a Zöllner's photometer. By these observations it is shown that in an hour and three quarters, on the evening of Jan. 1, the comet increased in brilliancy about seven-tenths of a magnitude, and diminished again to about its original lustre, simultaneous changes in the form of the nucleus being not less remarkable than the alteration in brightness.

In addition to these comets, it may be mentioned that two others of short period have passed through their perihelion, but owing to unfavourable circumstances have not been seen. One, Tempel's Comet of 1873, passed its perihelion on Nov. 20; the other, that of D'Arrest, made its closest approach to the Sun on Jan. 13 of this year. The theoretical brilliancy in both cases was very small, and when at the maximum the elongation from the Sun inconsiderable.

Lastly, Mr. Ellery telegraphs from Melbourne the discovery of a comet on Jan. 12, in the constellation *Grus*, but it seems not improbable that it will not be visible in the northern hemisphere.

W. E. P.

The Total Solar Eclipse of 1883, *May* 6.

This Eclipse was satisfactorily observed by all the astronomers who went out to Caroline Island for that purpose.

Circumstances prevented any regular expedition being sent out by this country, and the action of the Royal Society and the Solar Physics Committee was confined to photographic work. For this purpose Mr. Lawrance and Mr. Woods were sent to carry out a photographic attack which had been carefully and systematically planned by Mr. Norman Lockyer and Captain Abney. Through the courtesy of the American astronomers, Messrs. Lawrance and Woods accompanied the U.S. expedition, under the direction of Prof. E. S. Holden, of the Washburn Observatory, Madison.

The following photographs have been secured by them:—

1. Six good photographs of the corona, exposures varying from two to sixty seconds, giving coronal detail from near the limb to end of streamers. That the limit of the corona has been photographed is shown by the manner in which the light of the sky has impressed itself on the plate.

2. Three large photographs showing the details of the corona close to the limb.

3. Good photographs of the spectrum of the corona, showing a great number of coronal lines and very faint Fraunhoferic lines.

4. Photographs taken on a moving plate in integrating spec-

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troscope from one minute before to one and a half minute after totality, showing the most prominent lines of the *reversion* spectrum. These lines belong mainly to hydrogen.

5. Photographs taken with a grating before, during, and after totality. These show H and K near the limb throughout the whole of totality.

6. Photographs taken with a dense prism spectroscope before, during, and after totality. These photographs also give some of the prominent lines of the *reversion* spectrum.

7. Two photographs taken on plates sensitive to ultra-red rays. Results comparatively indifferent, on account of the absence of prominences.

The American astronomers also secured some important results. Prof. E. S. Holden's search for intra-Mercurial planets had a purely negative result. Dr. C. S. Hastings' spectroscopic observations are of great interest. He used a 60° prism attached to a 6¼-inch Equatorial, there being two total reflecting prisms placed outside the slit, so that the spectrum of two opposite sides of the Sun could be brought into juxtaposition and simultaneously examined. This was used to note the appearance of the 1474 K line. It was found that at the beginning of totality the 1474 K line on the eastern limb was about 12° in length and very bright, while on the western limb it was 4° in length and very faint. As the eclipse progressed this inequality vanished; the two lines became sensibly equal at mid-eclipse, while at the close of totality the conditions of the beginning were again noted, the line on the western limb being the longer and brighter. Since so large a change is much greater than would be due to the Moon's motion alone, the observation is regarded by Dr. Hastings as furnishing conclusive proof that the outer corona is in the main due to diffraction, that is, that the true corona is confined to a very narrow ring around the Sun, whilst the exterior portions are simply a diffraction phenomenon, the true corona being optically widened out by diffraction at the limb of the Moon. Dr. Hastings' conclusion has been challenged, and the solution of the question must probably depend upon observations at future total eclipses, or upon the success which may attend the efforts to photograph the corona without an eclipse.

The results obtained by the Continental astronomers MM. Janssen, Tacchini, Palisa and Trouvelot are no less important and interesting.*

From his spectroscopic observations M. Janssen concludes that "the basis of the coronal spectrum was formed by the complete Fraunhofer spectrum, and that, therefore, there exists in the corona, and above all in certain localities of it, an enormous amount of reflected light; and since we know that the coronal atmosphere is very rare, it follows that these regions

* *Comptes Rendus*, vol. xcii., No. 10.

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must abound in cosmic matter in the state of solid corpuscles, in order to explain this abundance of reflected sunlight."

The series of photographs obtained by M. Janssen are stated by him to show a much greater extension of the corona than was visible in the telescope; the photographic corona is well defined, and shows a large amount of detail.

M. Tacchini's spectroscopic observations are important, as he believes that he recognised in the spectrum of one streamer of the corona two of the bands—namely, that in the green and that in the yellow—so well known as characterising the spectra of comets. It will be of great importance if this observation can be confirmed at future eclipses.

On the whole the eclipse of 1883 was most successfully observed; and, apart from the purely solar phenomena, the evidence is pretty conclusive that no intra-Mercurial planet exists which is brighter than a fifth magnitude star.

The Transit of Venus, 1882.

The observations received consist of seventy-three observations of contact—viz., thirty-four at Ingress and thirty-nine at Egress—divided into the following groups :—

	Ingress.	Egress.
With 6-in. telescopes	16	11
„ 3-in. to 6-in. telescopes ...	11	23
„ less than 3-in. telescopes .	7	5

The observations were made at the following stations :—

INGRESS ONLY.

Madagascar; Cape of Good Hope, Royal Observatory, and at Montagu Road and Aberdeen Road; Natal, Durban, and Mauritius.

INGRESS AND EGRESS.

Jamaica, Barbados, Bermuda, Straits of Magellan, and two stations in Canada.

EGRESS ONLY.

Two stations in Canada, Melbourne, Adelaide, Hobart, and at eight stations in New Zealand.

The whole of the time determinations for these contacts are satisfactory, and the clock errors, rates, and local times of contacts, are now determined.

The latitudes and longitudes of the various stations are also determined, except for the station at the Straits of Magellan, the longitude of the neighbouring German station being required to

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complete this. The difference of longitude between Burnham and Mount Cook Observatory was carefully determined by Lieut.-Colonel Tupman, who also connected his station at Burnham by telegraph with Sydney. The signals received and sent by the Sydney Observatory have, however, not yet been received; but a determination of the difference of longitude between Mount Cook Observatory and Sydney is in progress, and the result expected by next mail. The differences of longitude between Mount Cook Observatory and the other New Zealand stations have all been determined. The tabular reductions are now in hand, and will be rapidly completed.

The telegraphic determinations of the longitudes of the Australian stations have long been under discussion, but no definite arrangements had been made until after the meeting of the Board of Visitors at the Greenwich Observatory in 1882, June, when the matter was placed in the hands of Mr. Stone. It was then found that Mr. Todd was willing and able, with the co-operation of Messrs. Ellery and Russell, to undertake the Australian work if an observer could be sent to Singapore. The distribution of the observers for the Transit of *Venus* had then been made, and some difficulty was experienced in finding anyone able and willing to undertake the work. Ultimately, although with some difficulty in the way of securing the necessary extension of leave of absence, the work was undertaken by Captain, now Major, Darwin, R.E., and it could not have fallen into more efficient hands. Captain Darwin while in Australia determined the difference of personal equation between the Australian observer and himself, and then returned *viâ* Singapore. Signals were exchanged between Captain Darwin at Singapore and Mr. Barrachi at Port Darwin on eight nights, and the results are very accordant. The telegraphic arrangements were in charge of Mr. Todd of Adelaide, and the whole of the arrangements of the Australian continent were under the direction of Messrs. Todd, Ellery, and Russell.

The longitude of Singapore had been determined by Dr. Oudemans, and Lieutenant Green of the U.S. Navy.

Taking the longitude of Madras $5^h\ 20^m\ 59^s{\cdot}35$ E., and giving double weight to Lieutenant Green's determination of the longitude of Singapore, we get—

				h	m	s
Singapore, East of Greenwich		...	...	6	55	24·69
Port Darwin	,,	...	...	8	43	22·21
Adelaide	,,	...	...	9	14	20·08
Melbourne	,,	...	...	9	39	53·83
Sydney *	,,	..	...	10	4	48 93

The longitude of the station at Madagascar (Nos Vey) was

* Mr. Russell, in a report just received, gives the longitude of Sydney $10^h\ 4^m\ 50^s{\cdot}81$ E. from seven years' observations of the Moon; Mr. Stone from two years' observations of the Moon, obtained $10^h\ 4^m\ 47^s{\cdot}32$ E.

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determined by chronometer runs to and from Natal, obtaining time at Natal direct from the Cape of Good Hope Observatory. The result is—

	h	m	s
Madagascar (Nos Vey) East of Greenwich	2	54	24·01

The difference of longitude between the Bermuda station and Washington was also determined by chronometer runs to and from New York, Washington time being telegraphed to New York; the result, which agrees very closely with the Admiralty determination, is—

	h	m	s
Bermuda (Gibb's Hill) West of Greenwich	4	19	20·45

Transits of Mercury, 1677–1881.

During the last few years a most important series of astronomical Papers prepared for the use of the *American Ephemeris and Nautical Almanac* has been published under the direction of Prof. Simon Newcomb. The objects of the series of papers are a systematic determination of the constants of astronomy from the best existing data, a re-investigation of the theories of the celestial motions, and the preparation of the tables, formulæ, and precepts for the construction of Ephemerides and for other applications of the results.

The first volume, which is just completed, consists of the following papers, some of which have already been noticed by the Council:—

1. "On the Recurrence of Solar Eclipses, with Tables of Eclipses from B.C. 700 to A.D. 2300," by Simon Newcomb.

2. "A Transformation of Hansen's Lunar Theory compared with the Theory of Delaunay," by Simon Newcomb aided by John Meier.

3. "Experimental Determination of the Velocity of Light made at the U. S. Naval Academy, Annapolis," by Albert A. Michelson.

4. "Catalogue of 1098 Standard Clock and Zodiacal Stars," prepared under the direction of Simon Newcomb.

5. "On Gauss's Method of Computing Secular Perturbations, with an Application to the Action of *Venus* on *Mercury*," by George W. Hill.

6. "Discussion of observed Transits of *Mercury* 1677–1881," by Simon Newcomb.

In addition to these, there is a preface by Prof. Newcomb giving a very able account of the present state of our theories of the celestial motions.

The concluding paper is devoted to a new and exhaustive discussion of observed Transits of *Mercury*, undertaken for the following reasons. The series of Transits of *Mercury* discussed

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by Le Verrier terminated with that of 1848. The fact that a third of a century of observations, far more accurate than any made in previous times, are now available is one reason for ascertaining whether any modification of his results is now possible. Transits of *Mercury* also help to decide several questions. Among these is the question of the uniformity of the Earth's rotation. The discrepancies between theory and observation in the Moon's secular acceleration, and the inequalities of long period of its longitude, give rise to the question whether the rotation of the Earth itself may not be variable. The question whether the apparent discrepancies in the Moon's motion are to be accounted for in this way can best be settled by observations on other rapidly-moving bodies. Transits of *Mercury* afford data also for the solution of those questions of the phenomena of contact which have to be interpreted and discussed in observations of Transits of *Venus*.

In the present paper Prof. Newcomb has endeavoured to work out, collate, and compare the results of theory and observation so that astronomers of the future may be able to see how they are to be reconciled without the necessity of going over the entire discussion from the beginning.

In the selection of observations available, considerable discrimination had to be exercised. The rule adopted was to retain all observations of known observers, and all made at well-known observatories or stations even when the observer was not known as an experienced astronomer, but as a rule to leave out of consideration those of unknown observers at stations whose longitude would be difficult to determine.

On the important questions of the distinction of phases and the methods of deducing the time of contact from the observations, there is of course a great deal to be said and various opinions may be formed. The author states that all experience concurs in showing that in actual observation no such distinct classification into the two phases of tangency of limbs and formation of the thread of light is possible. No tendency of the observations to group about any particular phase is found, and the conclusion arrived at is, that it is useless to attempt any investigation of phases, and that the proper course is to take an indiscriminate mean of the results of all observers irrespective of their description, except in those cases where there was something clearly different from true contact observed. Where observers assign different times, the phases corresponding to these times must be different, and these differences have to go in with the probable error of the final result. Upon this principle observations are rejected or corrected when it appears probable that they did not, or could not, correspond to the general mean.

In such an investigation, where the probable error of the observations is from 5^s to 10^s, absolute precision in the determination of the longitudes of the observers is not necessary. The Table of Longitudes of observing stations has been obtained from various published sources.

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The Tabular Summary of Observations is arranged in chronological order and contains the recorded observations of sixteen November, and seven May Transits of *Mercury*, giving the station, names of observers, description of phase, time as given by the observer (which is generally apparent time before 1800, and mean time since), reduction to geocentric phase, and concluded Greenwich mean time of geocentric contact.

Part II. of the paper is devoted to the computation of tabular elements. For the purpose of deriving the time of contact and other results from the tabular positions, Prof. Newcomb has adopted a method founded on Bessel's theory of eclipses. By this method the time of contact is defined as that when the observer is on the conical surface touching the planet and the Sun. Of the two circumscribing cones, that whose vertex is between the interior planet and the Earth will correspond to internal contact, and that whose vertex is between the planet and the Sun to external contact.

In the discussion of the results, which forms Part III., two important questions are considered :—

1. Do the Transits of *Mercury* prove or disprove the hypothesis of the variability of the Earth's axial rotation ?

2. What are the possible causes of the excess of motion of the perihelion of *Mercury ?*

In the discussion of the first question a term is introduced depending on hypothetical variations of the Earth's rotation. Taking as the standard of time the Earth's axial rotation between 1750 and 1850, and assuming that the observed inequalities in the Moon's mean motion are to be accounted for by actual inequalities in the Earth's rotation, then our measurement of time would be in error by amounts varying from 17 seconds in one direction to 17 seconds in the other direction between 1723 and 1881. A table is given showing the amount of these errors when interpolated to the times of the several observed Transits of *Mercury*. The values of Δt are assumed to be multiplied by a constant factor κ, and from the equations of condition, a value of κ is found which best satisfies the observations. If the hypothesis of perfect uniformity in the Earth's rotation be the true one, $\kappa = 0$; if the observed inequalities in the Moon's mean motion arise from the cause supposed, $\kappa = 1$; if κ should come out different from either zero or unity by an amount greater than its possible error, it would arise from errors of observation or a combination of both hypotheses. The value found is $\kappa = +0{\cdot}295$. The three cases of $\kappa = 0$, $\kappa = +0{\cdot}295$, and $\kappa = 1$, are then discussed, and a conclusion arrived at that "Inequalities in the motion of the Moon not accounted for by the theory of gravitation really exist, and exist in such a way that the mean motion of the Moon between 1800 and 1875 was really less than it was between 1720 and 1800."

In the discussion of the motion of the perihelion of *Mercury* some important results are arrived at. Adopting for the mass

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of *Mercury* $\frac{1}{7500000}$, which is a large diminution from Le Verrier's value, and for the mass of *Venus* $\frac{1}{408000}$, Prof. Newcomb concludes that the discordance between the observed and theoretical motions of the perihelion of *Mercury* first pointed out by Le Verrier really exists, and is, indeed, larger than he supposed. And in determining the excess of motion he concludes that "the observed centennial motion of the perihelion is greater by 43″ than the theoretical motion computed from the best attainable masses of the planet." In seeking for a possible explanation of this excess of motion, the author considers several arguments which have been brought forward—such as a possible term of very long period; Le Verrier's hypothesis of a planet or group of planets between *Mercury* and the Sun; the Zodiacal Light; a possible ellipticity of the Sun or his atmosphere; a ring round the Sun; any modification of the law of gravitation; Weber's electro-dynamic theory; all of which are rejected, and he concludes that, "assuming that we are still to look to a more exact determination of the astronomical character of the phenomena for a solution of the question, the necessary steps are, an exact determination of the mass of *Venus* from the periodic perturbation of the inner planets; an investigation of the secular motions of the planes of the orbits of those planets; and the comparison of the theoretical and observed secular motion of the perihelion of *Venus*."

In the concluding chapter on the Law of Recurrence of Transits of *Mercury*, Prof. Newcomb thus briefly indicates the high importance that should be attached to observations of that phenomenon. "Until the question of possible changes in the Earth's axial rotation shall be placed on a firm basis, or until the theory of the Moon's mean motion shall be so perfected that these changes can be determined with precision from observations of that satellite, transits of *Mercury* must be regarded with the greatest interest as affording independent determinations of the variations in question. The November Transits will long be most favourable for this purpose, for the reason that the series of observed November Transits extends back nearly a century before the first well-observed May Transit. It is to them, therefore, that we must principally look for light upon this question."

This discussion of Prof. Newcomb's forms a most important contribution to astronomy, and the results he arrives at, and the suggestions he makes, deserve the careful consideration of all astronomers. E. B. K.

The Constant of Aberration.

One of the most important contributions of recent years to observational astronomy comes from M. Magnus Nyrén, of the Imperial Observatory, Pulkowa, in the form of two remarkable

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and independent determinations of the Constant, or *Maximum* value, of Stellar Aberration.

Sure indications had been obtained, from other series of observations at Pulkowa, that the classic determination by W. Struve required an augmentation greatly exceeding in amount the probable error deduced by him. As long ago as 1872, M. Otto Struve admitted that a careful re-determination of this constant was very desirable, owing to the possible existence of small systematic errors caused by inequalities of short period in the rate of the clock employed by his father, and variations, also of short period, in the azimuthal position of the horizontal axis of the Transit instrument.

The seven stars used by W. Struve gave the following individual results:—

	Constant of Aberration.	Probable Error.
	″	″
v Ursæ Majoris	20·457	0·030
ι Draconis	20·479	0·022
δ Cassiopeiæ	20·456	0·046
o Draconis	20·403	0·023
b Draconis	20·504	0·032
Piazzi XIX. 371	20·395	0·033
β Cassiopeiæ	20·423	0·035

From which he derived the mean value $20''\cdot445\pm0''\cdot011$.

He subsequently considered the value $20''\cdot463\pm0''\cdot012$ to be a more accurate inference from the observations; but his reasons appear to have been not generally considered conclusive.

M. Nyrén takes notice of the fact, first pointed out by the late M. Yvon Villarceau, that in order to determine rigorously the Constant of Aberration it is indispensable to take account of the movement of translation of the Solar System. Attributing to the latter an annual value probably not exceeding the diameter of the orbit of the Earth, he finds the maximum effect upon the Constant of Aberration to be only $0''\cdot00006$, and that, therefore, there is nothing to fear from this cause in a result deduced from modern observations.

The instrument used by M. Nyrén was the same as Struve used. It had been supplied with a new micrometer eyepiece and sensitive level. The construction of the new micrometer enabled the inclination of the movable to the fixed wires to be corrected, which was not possible formerly. The fixed wires, about 37″ apart, were in groups of seven on either side of the central wire, from which they were distant about 110″. Struve's method of observing was strictly adhered to. A transit having been observed over the wires on one side of the field, the instrument was reversed and the star again observed over the same wires. The same being done on the other side of the meridian, a complete observation was obtained. There were two wires 37″ apart

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movable by the micrometer screw, and when it was necessary to use these wires, they were placed in the position required and not moved until the transits on that side of the meridian were completed.

During an observation the level was left resting on the pivots, and was not reversed except with the instrument. With each complete observation were 8 readings of the level—one before and one after each of the four transits. Care was taken to keep the level error within a few tenths of a second.

A clock by Dent, having a very regular rate of its own, was employed for the observations; but to guard against small variations of its rate it was compared with the normal clock of the Observatory before and after every complete observation.

The very serious matter of the instability of the azimuthal position of the axis of the Transit instrument was met by erecting marks east and west, distant 400 and 500 feet respectively from collimating objectives. By means of these marks the azimuth could be tested as often as required. It was found that the axis was liable to occasional disturbances in azimuth to the extent of $2''$ of arc in as many hours. A careful discussion of all the readings of the marks showed a small but discernible systematic movement, sufficient to introduce errors into the observed declinations. The marks were observed before and after every complete observation, and any observed change of azimuth was assumed to have taken place uniformly with the time during the interval.

Another point—apparently insignificant, but in reality of the utmost importance—was the illumination of the field. This was effected by an annular metallic elliptic reflector, placed in the central cube at an angle of 45° to the optic axis. The inclination of the reflector was slightly altered from its former position. Formerly the illumination of the field was obtained from a lamp suspended to the wall of the observing room, which was only effective when the telescope was in the vertical position. The illumination was now made so as to be independent of the position of the tube. This change would have no effect upon the deduced zenith distances so long as the illumination was precisely similar (*égal*) in the two positions of the horizontal axis; but it is otherwise if the field is illuminated in the two positions by reflection from different parts of the ring. All possible care was taken to neutralise this troublesome source of error.

Twenty-four stars were selected for observation, well placed in Declination (that is, culminating very close to, and north of, the zenith of the instrument), and pretty evenly distributed in the 24 hours of Right Ascension, so as to insure observations at all seasons of the year. The series commenced 1879 December 17 and terminated 1882 January 29, during which interval 566 complete observations were obtained. The period embraces two *maxima* and two *minima* * of aberration of each star; but it hap-

* The *minimum* means the *negative maximum*.

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pened that some of the winter stars were observed at only one *maximum* and one *minimum*.

All the observed declinations were reduced to the mean epoch 1881·0 by the *Tabulæ Pulcovenses*. The proper motions in declination for some stars were taken from Auwers' Fundamental Catalogue, or from Becker's Catalogue of 521 Bradley's stars. For the other stars, M. Nyrén himself deduced the proper motions from the best available data.

In the Equations of Condition the annual parallax of the star is introduced as one of the unknown quantities to be determined; but as the observations were made near the epochs of *maximum* aberration—and consequently when the coefficient of parallax was almost insensible—the introduction of the parallactic term was merely to obtain its effect upon the inferred Constant of Aberration.

Each complete observation furnishes an Equation of the form

$$ax + by + cz + n = 0,$$

where $a = -1$, $b =$ the coefficient of aberration, $c =$ the coefficient of parallax, $n =$ observed mean declination − tabular, $x =$ the correction to the adopted mean declination, $y =$ the correction required to the aberration 20″·445, $z =$ the parallax of the star.

M. Nyrén then gives, for each observation, the observed declination, and the values of n, b, and c.

The following table exhibits the mean result obtained from each star.

	R.A. h m	No. of Observations.		
β Cassiopeiæ ...	0 3	26	$y = +$ ″·045 + ·88z	± ″·067
δ Cassiopeiæ ...	1 18	44	+ ·081 + ·46	·039
χ Cassiopeiæ ...	1 26	22	+ ·224 + ·73	·054
2 H. Camel. ...	3 19	23	+ ·078 + ·49	·053
10 H. Camel. ...	3 55	11	+ ·181 + ·60	·085
B. D. 793 ...	4 13	21	+ ·041 + ·47	·062
11 Camel. ...	4 56	23	− ·148 + ·27	·058
37 Camel. ...	5 59	17	− ·018 + ·29	·095
2 Lyncis	6 9	24	− ·066 − ·11	·062
14 Lyncis ...	6 43	25	− ·004 + ·18	·059
15 Lyncis ...	6 47	30	− ·288 − ·06	·059
24 Lyncis ...	7 33	33	− ·138 − ·14	·060
υ Urs. Maj. ...	9 43	31	+ ·059 + ·04	·044
74 Urs. Maj. ...	12 24	18	+ ·072 − ·37	·050
75 Urs. Maj. ...	12 25	11	+ ·052 − ·38	·040
Radcliffe 3104 ...	13 46	10	− ·052 − ·51	·052
ι Draconis ...	15 22	41	+ ·218 + ·38	·023

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	R.A.	No. of Observations.	
	h m		
θ Draconis ...	16 0	35	$y = +\cdot177 + \cdot28\varepsilon \pm \cdot021$
b Draconis ...	18 22	38	$+\cdot186 + \cdot08$ $\cdot023$
o Draconis ...	18 49	41	$+\cdot071 + \cdot05$ $\cdot024$
Rad. 4511 ...	19 51	15	$+\cdot042 + \cdot54$ $\cdot080$
Rad. 5074 ...	20 56	10	$+\cdot103 + \cdot84$ $\cdot098$
B.A.C. 7387 ...	21 10	11	$+\cdot016 + \cdot76$ $\cdot112$
15 Cephei ...	22 0	6	$+\cdot318 + \cdot65$ $\cdot145$

Ten observations only were rejected, the observed Declinations being more than 0″·6 in error.

Weighting these in accordance with their probable errors, the mean is obtained,

$$y = +0''\cdot105 + 0\cdot19\pi \pm 0''\cdot0086,$$

where π is the mean parallax of the observed stars.

The author considers that this method of weighting the separate determinations would be proper if the observations, or rather the coefficient of aberration, were uniformly distributed in Right Ascension; but as this was not strictly the case, he divides the equations into four groups for each six hours of R.A., and thus obtains the following mean values:—

R.A.	h h 0 to 6,	$y = +0''\cdot066 + 0\cdot52\pi \pm 0''\cdot026$
	6 to 12,	$y = -0\cdot067 - 0\cdot02\pi \pm 9\cdot040$
	12 to 18,	$y = +0\cdot163 + 0\cdot16\pi \pm 0\cdot025$
	18 to 24,	$y = +0\cdot127 + 0\cdot13\pi \pm 0\cdot019$

Here the probable errors are inferred from the differences between these mean corrections and the separate corrections of the preceding table, taking into account the weights of the latter.

The author remarks, "The high degree of accuracy obtained in the individual observations justified the expectation that the groups thus formed would have been in better agreement. In fact the greatest discordance is between the two intermediate groups in which the individual observations have the smallest probable errors. It is therefore highly probable that the results are not free from systematic error." This inference is shown to be more probable by arranging the observations in monthly means in the order of time; it is then seen that from February to August 1881 the signs of the mean corrections are all *negative*.

M. Nyrén is not able to suggest any cause for systematic error other than temporary changes of personal equation of the observer, variations of latitude of the instrument, or lateral

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refraction, each of which would be likely to produce accidental rather than systematic errors.

The author finally adopts the arithmetic mean of the four groups, viz. :—

$$y = +0''{\cdot}072 + 0{\cdot}20\pi \pm 0''{\cdot}014,$$

and considers that the degree of uncertainty of this result is pretty accurately indicated by the probable error assigned—a conclusion in which few will be inclined to agree with him in the face of the above discrepancies of the R.A. groups.

The mean parallax of the stars (π) is, of course, unknown; but, considering that the stars that contributed the largest parallax factors were of less than the fifth magnitude, it cannot be doubted that π is extremely small.

The other series of observations from which this Constant has been inferred were also made at Pulkowa, by M. Wagner, with the large meridian Transit instrument, on the Right Ascensions of three circumpolar stars, in the years 1861–72, which are as yet only in part published. The accuracy of these observations surpass anything of the kind previously obtained.

The stars observed were α and δ *Ursæ Minoris*, and *Cephei* 51 (Hevelius). With few exceptions all the meridian passages were observed both by eye and ear and by the chronographic method. The Constant of Aberration has been inferred separately from the two series.

The observations, which were exceedingly numerous, were combined in groups of a fortnight each. After attributing to each transit a weight depending on the quality of the images and number of wires observed, Equations of Condition were formed precisely similar to the preceding [x, of course, now being the correction required to the adopted Mean Right Ascension], from which M. Nyrén obtains the following results :—

		No. of Culminations.*	
α Ursæ Minoris,	E. & E.	232, 207	$y = +0''{\cdot}033 \pm 0''{\cdot}005$
			$z = +0{\cdot}016 \pm 0{\cdot}005$
	Chronog.	218, 211	$y = +0{\cdot}044 \pm 0{\cdot}004$
			$z = +0{\cdot}005 \pm 0{\cdot}005$
δ Ursæ Minoris,	E. & E.	136, 144	$y = +0{\cdot}028 \pm 0{\cdot}023$
			$z = +0{\cdot}029 \pm 0{\cdot}011$
	Chronog.	133, 143	$y = +0{\cdot}021 \pm 0{\cdot}022$
			$z = +0{\cdot}042 \pm 0{\cdot}012$
Cephei 51 (Hev.)	E. & E.	123, 101	$y = +0{\cdot}068 \pm 0{\cdot}020$
			$z = +0{\cdot}032 \pm 0{\cdot}011$
	Chronog.	125, 110	$y = +0{\cdot}001 \pm 0{\cdot}019$
			$z = +0{\cdot}029 \pm 0{\cdot}011$

* The two numbers given in this column are the numbers of Upper and Lower Culminations respectively.

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Adopted Mean Correction of the Aberration Constant 20″·445 } +0″·038 ± 0″·003.

This probable error does not represent the degree of accuracy of the result, as is shown by taking account in the equations of nutation, proper motion, and possible instrumental variations.

We have, therefore, two new and entirely independent determinations of this Constant, viz. :—

20″·517 from the Prime Vertical observations of Declination;
20·483 from the Meridional observations of R.A.

M. Nyrén then gives the following summary of the various determinations that appear to him entitled to a high degree of confidence—all obtained at Pulkowa, but with three different instruments.

(*a.*) Vertical Circle. Observations of *α Ursæ Minoris* in declination.

Series by M. Peters	20″·507 ± 0″·021
„ Gyldén	20·469 ± 0·026
„ Nyrén	*20·498 ± 0·021
Mean for the Vertical Circle ...	20·495 ± 0·013

(*b.*) Meridional Transit. *α Ursæ Minoris.*

MM. Schweizer–Lindhagen	20″·498 ± 0″·012
M. Wagner	20·483 ± 0·012
Mean for Meridional Transit ...	20·491 ± 0·009

(*c.*) Prime Vertical Transit.

Series by W. Struve	20″·463 ± 0″·017
„ M. Nyrén	20·517 ± 0·014
Mean for Prime Vertical	20·490 ± 0·011

With equal weights, the mean of these is the definite result for the Constant of Aberration,

20″·492 ± 0″·006.

As recently suggested by Mr. A. M. W. Downing,† this value will probably be accepted by astronomers as hardly admitting of improvement from observations made in the Northern Hemisphere. G. L. T.

* Misprinted in the Memoir 20·495.
† *Observatory*, 1883, December.

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Mr. G. W. Hill's Paper on the Lunar Theory.

Mr. G. W. Hill has contributed to the *American Journal of Mathematics* an able paper, entitled, "On certain possible Abbreviations in the Computation of the Long-Period of the Moon's Motion due to the direct Action of the Planets."

It is well known that the calculation of the coefficients of these important inequalities has been hitherto regarded as extremely difficult. The author shows how, by the use of his method, this calculation can be greatly facilitated. The work may be divided into two portions, independent of each other. In one, the object is to develop, in periodic series, certain functions of the Moon's coordinates, which in number do not exceed five. This portion is the same whatever planet is considered to act, and hence may be done once for all.

In the other portion we have to find the coefficients of certain periodic terms in the development of certain functions, five also in number, which involve the coordinates of the Earth and planet only. And this part of the work is very similar to that in which the perturbations of the Earth by the planet in question are the things sought. As the multiples of the mean motions of these two bodies, which enter into the expression of the argument of the inequalities under consideration, are necessarily large, approximate values of the coefficients may be obtained by means of semi-convergent series.

The author's method of finding these coefficients is based on that given by Cauchy in Tome xx. of the *Comptes Rendus*, but he introduces a modification of the method, founded on a very elegant solution of a set of simultaneous equations, by means of which a great saving of labour is effected.

Auwers' Mittlere Oerter von 83 Südlichen Sternen.

This important paper is a continuation of the same author's larger work, entitled *Fundamental-Catalog für die Zonenbeobachtungen am Nordlichen Himmel*, published in 1879 as No. XIV. of the series *Publicationen der Astronomischen Gesellschaft*, and forms No. XVII. of the same series. The *Fundamental-Catalog* extended to $-10°$ Dec., and this addendum is intended to supply zero points for the zone observations of stars situated between $-10°$ and $-32°$ Dec.; of the new stars seventy-nine being between these limits of declination, and the remaining four lying between the Equator and $-10°$ Dec.*

The authorities for the star places, in addition to those used

* This catalogue of 83 Southern stars was first published in the *Vierteljahrsschrift der Astron. Gesellschaft*, 1880, p. 280, but without any details.

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for the *Fundamental-Catalog*, are the Cape Catalogue for 1860, the Williamstown Catalogue for the same epoch, the First Melbourne General Catalogue for 1870, Safford's Right Ascensions of 505 stars from the Cambridge (U.S.) Observations 1862–1865, the Washington Catalogue for 1860, and the Washington Transit-Circle Observations 1872 to 1875. And Dr. Auwers' general method of proceeding has been to find the corrections applicable to the Greenwich Nine-Year Catalogue, the Washington Catalogue (1860), the Cape, Melbourne, Williamstown, and Cambridge (U.S.) Catalogues, to reduce them to the system formed from a combination of the Greenwich First and Second Seven-Year Catalogues. Through the medium of this latter system the above-mentioned Catalogues are referred to the Pulkowa Catalogue for 1865 (which has been adopted as the fundamental system for the zone observations), and the other Catalogues which have been used in the investigation are also referred to the same system by the application of the requisite corrections. The adopted mean place for 1875·0 of each star is then deduced by taking the mean according to weight of its place found from each Catalogue used, the Catalogue place having been reduced to the fundamental system. The greatest normal weight (7) is given to Pulkowa 1865, the next highest (4) to Pulkowa 1845 and Greenwich 1872, and smaller weights to the other Catalogues.

On looking over the interesting Tables for reduction of the different Catalogues to the fundamental system, on p. 27 of this memoir, it is somewhat remarkable that the great majority of the Catalogues require corrections *of the same sign* to reduce them to the Pulkowa system for 1865. This is noticeable all through the range of declination for the R.A.'s, and for the declinations it is very striking for stars south of the Equator. With regard to the R.A.'s, it may perhaps be pointed out that Nyrén has found (*Das Æquinoctium für* 1865·0, p. 31) that the difference of Equinox for Pulkowa (1861–1870) *minus* Greenwich (1836–1870) is as much as $+0^s{\cdot}064$. Further, it is to be remarked that the geographical position of Pulkowa is somewhat unfavourable for determination of the Equinox, owing to the low altitude of the Sun when culminating south of the Equator. Dr. Auwers himself recognises the probability of error in the declinations of the Pulkowa system for stars south of the Equator, which will perhaps be more clearly seen by comparing this system with Auwers' Mean System of Declinations (*Ast. Nach.* No. 1536), or with the system deduced by Boss (cf. *Observatory*, vol. iii. p. 479).

Although, therefore, astronomers will probably differ in opinion as to the accuracy of the fundamental system adopted for reduction of the zone observations of the *Astronomische Gesellschaft*, there can be no doubt but that Dr. Auwers has done much to secure the success of this great undertaking by the care and skill which he has bestowed on the formation of these fundamental Catalogues, without which it would be impossible to bring the work to a satisfactory termination. A. W. M. D.

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The Moon's Semi-diameter.

Prof. H. M. Paul, formerly assistant at the United States Naval Observatory, and late Professor of Astronomy at the University of Tokio, has published in an Appendix to the *Washington Observations for* 1879, a determination of the Moon's semi-diameter from two occultations of the *Pleiades*.

The two sets of occultations occurred in 1877 and 1879, and were observed with the 9·6-inch Equatorial, employing a power of 132 for most of the stars. All the immersions in both sets were at the bright limb of the Moon, and all the emersions at the dark limb.

For the 1877 observations the atmosphere was unsteady, the stars and the limb of the Moon being blurred and trembling. They were made at zenith distances of 71° to 53° and the immersions were very unsatisfactorily observed, Prof. Paul remarking that "he did not observe what he could be sure was an actual occultation of a single one of the seven stars at the bright limb, but he struck the observing key when the star seemed to lose itself in the light of the Moon." The emersions were more satisfactory.

The observations of 1879 were made when the atmosphere was extraordinarily steady, so much so that the author states that the condition of the images as regards steadiness and excellence of definition was such as he had never seen before at that place.

Prof. Paul's object was to deduce from the observations a correction to the ephemeris semi-diameter of the Moon, and also, as a necessary accompaniment, corrections to its Right Ascension, Declination, and Horizontal Parallax.

The relative positions adopted for the stars were those as determined by Wolf's differential measures applied to Newcomb's position of η *Tauri*, the proper motions depending upon a comparison of Wolf with Bessel, and the common proper motion of the group assigned by Newcomb.

Each set of occultations is discussed separately, and a table is given in which the residuals are arranged according to the grouping of the points of occultation round the Moon's limb, also with reference to bright and dark limb, and also with reference to the relative magnitudes of the stars.

In the discussion of the results of 1877, Prof. Paul calls attention to a curious relation between the residuals and the relative magnitudes of the stars. There is shown to be a difference of about 1·″4 in the resulting values of the semi-diameter if the stars are divided into two groups: one, those brighter than the mean magnitude, the other, those fainter, the *brighter* giving the *smaller* value of the semi-diameter. He concludes that the resulting corrections from the 1877 occultations cannot be

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considered as of much value on account of the probability of constant errors of larger magnitude than the resulting probable errors would indicate.

The 1879 observations give far more satisfactory results. They show that Wolf's positions of the stars are probably the best available at present.

The observations also show that the general figure of the Moon's limb must be almost a perfect circle. The deduced correction to Hansen's value of the mean semi-diameter is,

$$-1''\cdot69 \pm 0''\cdot12,$$

and the resulting value of the mean semi-diameter of the Moon as given by the fourteen occultations of 1879 is,

$$15'\ 31''\cdot78 \pm 0''\cdot12.$$

Prof. Paul next considers Airy's determination of the occultation semi-diameter from the discussion of 296 scattered occultations observed at Cambridge and Greenwich between the years 1830 and 1860. These give the following results, with the probable errors derived by Prof. Paul from further consideration of Airy's paper:—

From 132 Immersions at Dark Limb	15′ 32″·68	± 0″·16
64 Emersions at Dark Limb	15 32·28	0·24
51 Immersions at Bright Limb	15 34·08	0·29
49 Emersions at Bright Limb	15 36·19	0·31

Comparing these results with his own, he concludes that it is impossible to determine the position of the Moon's centre by meridian or altazimuth observations with any such degree of accuracy as that of a star, and that such determinations of the Moon's position are not of much value for the accurate determination of the Moon's occultation semi-diameter by means of scattered occultations. Prof. Paul considers the value that he has obtained as the most reliable thus far determined.

In conclusion, he suggests that the only way to observe occultations at the bright limb of the Moon, so as actually to *observe the real occultation*, is to use such a high magnifying power as to make a decided difference in colour between the star and the Moon's limb; and for a more accurate determination of the figure of the Moon's limb, he proposes heliometer measures of the diameter in the direction of the line of cusps near the time of Full Moon, when the position-angle of this line is changing very rapidly; but the mountain ranges on the limb, which would have a varying height according to the libration, would probably render such measurements uncertain.

The careful manner in which Prof. Paul has discussed his observations renders his paper a valuable contribution to the

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subject, and his result for the mean semi-diameter of the Moon will probably be received by astronomers with confidence.

E. B. K.

Dr. Meyer's Observations of Saturn.

Some interesting researches on the Planet *Saturn* have been undertaken at the Geneva Observatory by Dr. W. Meyer.

In 1880 and 1881 the dimensions of the planet were micrometrically measured with the 10-inch Refractor, and the results for 1881 have been published in the *Astronomische Nachrichten*, No. 2517. The dimensions were measured on twenty-two evenings in such a manner that every dimension is the result of an independent series of observations. An attempt was made to determine the question of the excentric position of the ball within the rings, but without result.

The following are the resulting dimensions of *Saturn* for the mean distance 9·5389.

	1880.	1881.	Diff.	±	No. of Obs.
	″	″		″	
Exterior diameter of the bright ring	40·43	40·35	−0·08	0·038	22
Diameter of the ring in the middle of Cassini's division		34·48		0·098	4
Interior diameter of the bright ring	26·30	26·05	−0·25	0·059	21
„ „ dusky ring	21·15	21·13	−0·02	0·085	5
Distance between the extremity of the ring and the planet on the west	11·59	11·34	−0·25	0·026	20
Ditto on the east	11·41	11·30	−0·11	0·038	20
Equatorial diameter	17·40	17·77	+0·37	0·028	21
Polar diameter	16·18	16·12	−0·06	0·055	20
Compression	$\frac{1}{14·5}$	$\frac{1}{10·7}$			

These measures may, in some respects, be compared with some more recent observations on *Saturn's* rings made by Prof. Otto Struve, published in the *Astronomische Nachrichten*, No. 2498. The observations were made in the year 1882 with the 14-inch Refractor of the Pulkowa Observatory, reduced to the mean distance 9·5389, and corrected for phase. Employing the usual designations A, B, and C for the outer and inner bright rings and the dusky ring.

Distance from Limb of Planet.

	Preceding Ansa.	Following Ansa.	Diff.
	″	″	″
Inner boundary of C	1·44	1·53	+0·09
Inner boundary of B	3·72	3·61	−0·11
Outer boundary of B	8·27	8·13	−0·14
Outer boundary of A	11·20	11·20	0·00

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Professor Struve compares the mean of these measurements with the series he made in 1851 with the following satisfactory result:

Distance from Limb of Planet.

	1851.	1882.	Diff. 1882–1851.
Inner boundary of C	1".61	1".49	−0".12
Inner boundary of B	3·64	3·66	+0·02
Outer boundary of B	8·24	8·20	−0·04
Outer boundary of A	11·03	11·20	+0·17

Dr. Meyer's observations put into the same form, and compared with Prof. Struve's, are—

Distance from Limb of Planet.

	Meyer (1881).	Struve. (1882).	M−S
Inner boundary of C	1·68	1·49	+0·19
Inner boundary of B	4·14	3·66	+0·48
Outer boundary of B	8·36	8·20	+0·16
Outer boundary of A	11·29	11·20	+0·09

Dr. Meyer remarks that the interior diameter of the bright ring (B) could not be measured with very great accuracy, since the interior edge of the ring did not appear sharply bounded.

Considering that Dr. Meyer's observations were measures of the diameters of the rings, and Prof. Struve's distances were from the limb of the planet to the edge of each ring, the agreement is satisfactory.

In 1882 it was pointed out by Prof. Schiaparelli that Encke's fine division in the outer ring was not symmetrical on the two sides. On 1881 November 15 he noted that the line was well seen in the following ansa, and divided the outer ring almost exactly in half. On the preceding ansa the division was seen with difficulty, but it was not in the middle of the ring, but nearer the outer limb, and divided the entire breadth into two parts, of which one was twice the width of the other. Dr. Meyer has confirmed Prof. Schiaparelli's observation in every particular. Further observations at Milan, in January and February 1883, confirm those made in 1881. This very strange feature does not appear to have been observed except at Geneva and Milan.

Dr. Meyer has published in the *Astronomische Nachrichten*, No. 2527, a very interesting investigation of the commensurabilities of the revolution periods of the satellites of *Saturn* with the periods which satellites would have if revolving at the distances of certain divisions in the ring; and he finds that,

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including Cassini's division, there are seven places where the satellites would unite to exercise a perturbing influence on the members of the ring system. If the fact of Encke's division being excentric to the other divisions in the ring be established, the problem will be very much complicated.

Continuing his researches, Dr. Meyer has given in the *Astronomische Nachrichten*, No. 2528, a new determination of the orbits of all the satellites except *Mimas* and *Hyperion*; the former he was able to observe five times, and the latter not at all. The following are the mean distances and periods, which are closely accordant with received values:—

	Mean Distance.	Period.
	"	d h m s
Enceladus	34·3501	1 8 53 6·92
Tethys	42·7514	1 21 18 25·62
Dione	54·7574	2 17 41 9·29
Rhea	76·4838	4 12 25 11·57
Titan	176·9102	15 22 41 23·16
Iapetus	514·7108	79 7 49 24·84

Dr. Meyer has also calculated the mass of *Saturn* from each one of the satellites, making due allowance for the influence of the Sun, the rings, and the polar flattening of *Saturn*, with the following results:—

	Mass.	Weight.
Enceladus	0·000284213	1·0
Tethys	0·000288215	4·4
Dione	0·000288531	9·1
Rhea	0·000288898	15·5
Titan	0·00028704	73·1
Iapetus	0·000285849	48·6

The mean mass which results according to the above weights is

$$\text{Mass} = \frac{1}{3482{\cdot}9 \pm 5{\cdot}5}.$$

This value agrees almost exactly with that recently obtained* by Prof. Asaph Hall from observations of *Iapetus* $= \frac{1}{3482{\cdot}2}$, and differs very little from Jacob's determination of the mass from elongations of *Titan* $= \frac{1}{3487{\cdot}2}$. E. B. K.

* *Monthly Notices*, vol. xliv., p. 7.

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Schiaparelli's Observations of Mars.

During the last ten or twelve years the physical features of the planet *Mars* have received a large amount of attention from observers. The combination of the drawings of Mr. Dawes by Mr. Proctor, so as to form a chart of the whole surface of the planet, the features of which were named on the hypothetical view that the dark markings were seas and the light spaces land, was of undoubted value as affording a fairly reliable and ready means of identifying any particular feature. In 1875 Dr. Terby, of Louvain, published his comparative examination of all the physical observations made of *Mars* from the time of Fontana, in 1636, to the year 1873, which he entitled *Aréographie*. In this important memoir he discussed and correlated all available drawings by reference to Mr. Proctor's chart, and thus made an important advance in our knowledge of the true features of the planet. Several series of drawings of considerable value have been made at recent oppositions—notably the exquisite Madeira series of Mr. Green; and two or three charts of the planet have been published, which represent the physical features with perhaps more detail than was possible from the series of drawings Mr. Proctor used.

At the very favourable opposition of 1877, Prof. G. V. Schiaparelli devoted a considerable amount of attention to the planet, the results of which he published in Series 3, vol. ii. *Atti della Reale Accad. dei Lincei*, entitled, "Osservazioni astronomiche e fisiche sull' asse di rotazione e sulla topografia del pianeta *Marte*." Not only was the position of the planet's axis determined, the areographic places of sixty-two fundamental points fixed, upon which a chart prepared from a long series of drawings could be reliably laid down, but a very elaborate and detailed description was given of every portion of the surface visible at that opposition. The memoir was accompanied by only four drawings, enough, however, to show a very close agreement with other drawings of similar phases. The map, given only in outline, however, showed many new features in the form of fine lines, presenting the appearance of a sort of network arrangement. These canals appear to be confirmed in many respects by careful comparison with other drawings, wherein some of them appear either as fine lines, or as the boundary lines of different tints or shadings.

At the opposition of 1879, Prof. Schiaparelli continued his researches on the planet with the same exhaustive care and attention which he had devoted in 1877. The results, contained in vol. v. of the same *Transactions*, form one of the most important works yet published on the subject. The position of the axis of rotation was determined from eighty-nine measurements of the

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south polar spot, and by combining the results of this series with those obtained in 1877 the coordinates of the projection of *Mars'* north pole on the celestial sphere were found to be

R.A. = 318° 7'·8, Decl. = + 53° 37'·1, Epoch 1880·01.

In the two oppositions of 1877 and 1879, 482 observations were obtained, which fix the areographical places of 114 fundamental points. Upon these observations a new chart was constructed, which, unlike that published in the first memoir, showed the relative intensity of the markings.

This memoir is accompanied by too few copies of the original drawings, for, as the map shows an increase in the number of the fine lines which formed so conspicuous a feature in the earlier chart, and which observers have found so difficult to identify, it is the more necessary for that purpose to possess copies of as many of the original drawings as possible for comparison with others. And the importance of this is now conspicuous, for from his observations of *Mars* at the opposition of 1882, Prof. Schiaparelli has constructed a new chart, which presents some most novel features. "The 'canals' of 1877 and 1879 are scarcely recognisable, but their place is more than supplied by a great number of narrow straight lines, distributed for the most part in pairs of parallel lines, distant from each other from 5° to 10° of a Martial great circle, and intersecting the equatorial regions in every direction. These lines are perfectly hard and distinct, each is perfectly uniform in breadth and intensity from end to end, and the majority are represented as being as distinct and dark as are the large markings that are shown on all the best drawings of the planet." There appears to be as yet no confirmation of the duplication of the "canals" thus indicated. But leaving the 1882 observations to be confirmed or not in the future, the high value of Prof. Schiaparelli's researches on the planet cannot be underrated. The determination of the position of so many well-recognised points on the surface of *Mars* by micrometric measurement forms the foundation of a far more accurate chart than was previously possible. It is very probable that some of the disputed canals have been represented too strongly, and that they are only discernible with an exceptionally clear atmosphere. Mr. Burton's observations of *Mars* in 1879 have confirmed some of them. A drawing of the "Terby Sea" by Dr. Lohse, recently published in vol. iii. of the *Potsdam Observations*, represents it as a square, three angles of which correspond with the points of junction of three canals shown in the Milan drawing; and it is probable that many other new features discovered by Prof. Schiaparelli will be thus confirmed.

With regard to the nomenclature adopted in maps of *Mars*, it is most desirable that there should be some agreement established among astronomers on the question. The principle

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adopted by Mr. Proctor, of designating the "lands and seas" by the names of astronomers, was provisionally a convenient one, and this was continued by Mr. Green and M. Flammarion in their maps, but with modifications. Prof. Schiaparelli has adopted the divisions of land and water, but selected his names from ancient geography and history, and the confusion in the nomenclature thus introduced renders the discussion of any particular region of the planet rather difficult. It is desirable that these different systems should not continue, and that some agreed nomenclature should be generally adopted. E. B. K.

Dr. Huggins' Method of Photographing the Solar Corona without an Eclipse.

In our last Report we mentioned the experiments of Dr. Huggins with this object in view, and also his results up to that time, which seemed to show that he had succeeded in getting an impression of the Corona upon his plates.

During the past year he has continued his work on the Sun with a more suitable form of apparatus (which will be found described in the Report of his Observatory), and has obtained photographs on a larger scale, and in several respects freer from the presence of possible sources of instrumental error. The original negatives were placed in the hands of Mr. Lawrance and Mr. Woods (the photographers who went to Caroline Island), to be compared by them with the photographs taking during the eclipse of May 6. Bad weather in this country prevented any photographs being taken here on that day.

Mr. Lawrance reports "the outline and distribution of light in the inner Corona of April 3 is very similar to that on our plate which had the shortest exposure; the outer Corona is, I think, hidden by atmospheric glare. As a result of the comparison, I should say that Dr. Huggins' Coronas are certainly genuine as far as 8′ from the limb."

Careful examination of three of the negatives taken on April 3, May 31, and June 6 show a large rift near the north pole similar to the rift which is so conspicuous a feature in the eclipse photographs.

A committee has been appointed by the Royal Society for the purpose of taking photographs of the Sun by Dr. Huggins' method, at some place of high elevation. If photographs can be taken under these circumstances to show the Coronal details with sufficient distinctness, it is scarcely possible to overvalue their importance with reference to the advance of our knowledge of that which exists immediately about the Sun.

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The Potsdam Physical Observatory.

The third volume of the publications of this Observatory has recently appeared, and it contains some interesting researches.

Dr. Oswald Lohse has continued his physical observations of *Jupiter* and *Mars;* those of *Jupiter*, ranging from 1878 to 1881, are accompanied by several drawings. The Red Spot was carefully observed, and a series of micrometric measurements were made of its distance from the limb.

An important series of photographs of *Jupiter* were taken with the 11-inch Refractor, and Dr. Lohse succeeded in photographing the Red Spot, two Equatorial Belts, and the North Polar Zone. The photographs appear to have been so successful that the ellipticity of the planet could be fairly determined by measurements of the photographic image.

A short series of observations of *Mars* in 1879 is accompanied by a few very carefully executed drawings.

The second paper in this volume is an investigation of the mass of *Jupiter*, by Dr. P. Kempf. He has employed for this purpose the observations of differences of R.A. of *Jupiter* and Satellites III. and IV., made by Vogel in 1868 to 1870, and some observations of Satellite IV. made by Airy in 1832 to 1836, which he has reduced anew. Comparing the different values of the mass of *Jupiter*, obtained from observations of the satellites and from perturbations, the averages of which are

From observations of the satellites ...	$\mu = 1047{\cdot}817$
From perturbations	$\mu = 1047{\cdot}583$

Dr. Kempf adopts the mean of these as a final result,

$$\mu = 1047{\cdot}700.$$

In the third paper Dr. Vogel and Dr. Müller have published an extensive series of spectroscopic observations of 4051 stars, down to the 7·5 magnitude, in the zone $-1°$ to $+20°$ Declination, the character of the spectra being briefly indicated.

Dr. Gustav Müller has published in this volume an important photometric investigation of the extinction of light by our atmosphere. The observations were confined to comparisons of five stars—*α Aurigæ*, *α Cygni*, *η Ursæ Majoris*, *δ Persei*, and *α Tauri*, at different altitudes with *Polaris*. A Zöllner photometer was employed throughout. For zenith distances greater than 80° *Polaris* was not used, but planets were observed when rising and setting, and the extinction was deduced from the differences between consecutive readings of the intensity circle. The final result arrived at is that the zenithal brightness of a star is 0·8250 of its light before incidence, or in other words, the atmosphere absorbs $\frac{7}{40}$ of the original light. This result is in very satisfactory accordance with the researches of Bouguer,

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Seidel, and lately by Prof. Pritchard, as may be seen by the following comparison :—

Ratio of the zenithal light of a star after absorption to the light before incidence :—

Bouguer	0·8123
Seidel	0·8039
„ (second determination) ...	0·7942
Pritchard (Oxford observations) ...	0·791
„ (Cairo „) ...	0·843
Müller	0·8250

Finally, the author concludes from the discussion of the observations, that Lambert's extinction theory is sufficient to satisfy the results, so long as no greater accuracy than 0·09 to 0·10 of magnitude is desired.

Longitude Determinations.

The United States Hydrographic Office has published an important series of Telegraphic Determinations of Longitudes in Japan, China, and the East Indies. The longitudes of very nearly all the prominent positions in the East Indian Archipelago, on the shores of the China and Japan Seas, and the Western Pacific Ocean, have been referred to one of the following points, viz.: Singapore, Batavia, Manilla, Hong Kong, Shanghai, Nagasaki, and Yokohama; and were the longitudes of these initial points known exactly, further work would be unnecessary. But the longitudes of these Secondary Meridians, which have been accepted for the last forty years, have depended mostly upon observations of Moon culminations, and liable to an uncertainty of from one to four minutes of arc.

The expedition sent out by the United States was under the command of Lieutenant-Commander Green, U.S.N., who is well known in connection with longitude work. The object was to form a chain of longitudes through Yokohama, Nagasaki, Wladiwostok, Shanghai, Amoy, Hong Kong, Manilla, Cape St. James, Singapore, Batavia, connected with Madras.

The work was carried out in its entirety, and the following important corrections to be applied to the Secondary Meridians adopted by the British Hydrographic Office, were made :—

Place.	Correction.
Batavia (Observatory)	+30″
Singapore (Fort Fullerton)	− 3
Manilla (Cathedral)	− 5
Hong Kong (Cathedral)	− 6·5
Shanghai (British Consulate Flagstaff) ...	+29
Nagasaki (Minage Point)	0·0
Yokohama (Hospital Square)	+30·5

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The last volume of the *Monthly Notices*, contains two interesting papers on longitude determinations in South Africa. The first, by Dr. Gill, being a preliminary account of a Telegraphic Determination of the Longitude of the Royal Observatory, Cape of Good Hope. The paper contains an account of the operations for connecting the longitudes of Aden and the Cape, which were successfully carried out; and though the definitive results are not yet published, the determination is one of very great importance.

A series of secondary longitudes on the east coast of Africa, determined by Mr. Finlay while engaged in the larger work connecting the Cape with Aden, forms the other paper in the *Monthly Notices* referred to. Mr. Finlay thus determined the longitudes of Delagoa Bay, Quilimane, Mozambique, and Zanzibar.

The proposed Unification of Longitudes and Time.

At the meeting of the International Geodetic Association at Rome in October last, the question of the adoption of a common initial meridian for reckoning longitudes and time was discussed. Delegates from the following fifteen States took part in the discussion, viz.:—Austria, Bavaria, Belgium, Darmstadt, France, Great Britain, Hamburg, Italy, Norway, Prussia, Roumania, Russia, Spain, Switzerland, and the United States. After receiving the Report of the Permanent Commission, which recommended the adoption of the meridian of Greenwich, the Conference adopted the following resolutions, by 30 votes against 1 abstention:—

"*Résolutions de l'Association Géodésique Internationale concernant l'Unification des Longitudes et des Heures.*

"La septième Conférence générale de l'Association Géodésique internationale réunie à Rome, à laquelle ont pris part des représentants de la Grande-Bretagne, ainsi que les directeurs des principales éphémérides astronomiques et nautiques, et un délégué du Coast and Geodetic Survey des Etats-Unis, après avoir délibéré sur l'unification des longitudes par l'adoption d'un méridien initial unique, et sur l'unification des heures par l'adoption d'une heure universelle, a pris les résolutions suivantes:—

"I. L'unification des longitudes et des heures est désirable autant dans l'intérêt des sciences que dans celui de la navigation, du commerce et des communications internationales; l'utilité scientifique et pratique de cette réforme dépasse de beaucoup les sacrifices en travail et les difficultés d'accommodation qu'elle entraînerait. Elle doit donc être recommandée aux gouvernements de tous les Etats intéressés, pour être organisée et con-

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sacrée par une Convention internationale, afin que, désormais, un seul et même système de longitudes soit employé dans tous les instituts et bureaux géodésiques, du moins pour les cartes géographiques et hydrographiques générales, ainsi que dans toutes les éphémérides astronomiques et nautiques, à l'exception des données pour lesquelles il convient de conserver un méridien local, comme pour les éphémérides de passage, ou de celles qu'il faut indiquer en heure locale, comme les établissements de port, etc.

"II. Malgré les grands avantages que l'introduction générale de la division décimale du quart du cercle dans les expressions des coordonnées géographiques et géodésiques, et dans les expressions horaires correspondantes, est destinée à réaliser pour les sciences et pour la pratique, il paraît justifié, par des considérations essentiellement pratiques, d'en faire abstraction dans la grande mesure d'unification proposée dans la première résolution.

"Cependant, pour donner en même temps satisfaction à des considérations scientifiques très-sérieuses, la Conférence recommande, à cette occasion, d'étendre, en multipliant et en perfectionnant les tables nécessaires, l'application de la division décimale du quart de cercle, du moins pour les grandes opérations de calculs numériques, pour lesquelles elle présente des avantages incontestables, même si l'on veut conserver l'ancienne division sexagésimale pour les observations, pour les cartes, la navigation, etc.

"III. La Conférence propose aux Gouvernements de choisir, pour méridien initial, celui de Greenwich, défini par le milieu des piliers de l'instrument méridien de l'observatoire de Greenwich, parce que ce méridien remplit, comme point de départ des longitudes, toutes les conditions voulues par la science, et que, étant déjà actuellement le plus répandu de tous, il offre le plus de chances d'être accepté généralement.

"IV. Il convient de compter les longitudes à partir du méridien de Greenwich dans la seule direction de l'Ouest à l'Est.

"V. La Conférence reconnaît pour certains besoins scientifiques, et pour le service interne des grandes administrations des voies de communications, telles que celles des chemins de fer, lignes de bateaux à vapeur, télégraphes et postes, l'utilité d'adopter une heure universelle, à côté des heures locales ou nationales qui continueront nécessairement à être employées dans la vie civile.

"VI. La Conférence recommande, comme point de départ de l'heure universelle et des dates cosmopolites, le midi moyen de Greenwich, qui coïncide avec l'instant de minuit ou avec le commencement du jour civil sous le méridien situé à 12^h ou à 180° de Greenwich.

"Il convient de compter les heures universelles de 0^h à 24^h.

"VII. Il est désirable que les Etats qui, pour adhérer à l'unification des longitudes et des heures, doivent changer de

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méridien, introduisent le nouveau système de longitudes et d'heures le plus tôt possible.

Il importe également que le nouveau système soit introduit sans retard dans l'enseignement.

"VIII. La Conférence espère que, si le monde entier s'accorde sur l'unification des longitudes et des heures, en acceptant le méridien de Greenwich comme point de départ, la Grande-Bretagne trouvera dans ce fait un motif de plus pour faire, de son côté, un nouveau pas en faveur de l'unification des poids et mesures, en adhérant à la Convention du Mètre du 20 mai 1875.

"IX. Ces résolutions seront portées à la connaissance des Gouvernements et recommandées à leur bienveillante considération, en leur exprimant le vœu qu'une Convention internationale, consacrant l'unification des longitudes et des heures, soit conclue le plus tôt possible par les soins d'une Conférence spéciale, telle que le gouvernement des Etats-Unis l'a proposée."

The French delegates, however, whilst accepting the resolutions as a whole, voted against those articles which involve the adoption of the meridian of Greenwich, proposing instead of this meridian one through the island of Ferro, defined as being 18° west of Greenwich. The question was also raised as to whether the reckoning of International time should commence at Greenwich noon (as in astronomical reckoning) or at Greenwich midnight (as in civil reckoning), and it was ultimately decided by a majority of 20 against 8 to recommend the adoption of the former. It is proposed to submit these recommendations of the Geodetic Conference for ratification by a special Congress, which the Government of the United States has invited to meet at Washington on Oct. 1 next.

Meanwhile the first practical step towards the unification of time has been taken by the Railway Companies of the United States and Canada, which have adopted throughout their system of railways, 100,000 miles in length, standard time reckoned from four meridians, respectively 5^h, 6^h, 7^h, and 8^h west of Greenwich, and thus agreeing with Greenwich mean time as to the minutes and seconds. Time signals in accordance with this system were commenced by the Yale College Observatory, on Nov. 18, when the new standard "Eastern time," 5^h west of Greenwich, was substituted for New York City Hall time, which is $3^m\ 58^s{\cdot}4$ fast of "Eastern time."

Harmonic Analysis of Tidal Observations.

At the meeting of the British Association at Southampton in 1882, Prof. Adams and Prof. G. H. Darwin were appointed a committee for the Harmonic Analysis of Tidal Observations. The old method of reducing tidal observations consisted

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in grouping the heights of high and low tide according to the times after the Moon's transit at which it is slack water. Observations of maxima are unsatisfactory, both because the determination of the time of maximum is affected with uncertainty, and because sufficient data are not thus afforded for a good determination of the tidal constants. For the harmonic analysis a continuous tidal record is necessary, and the data which are analysed consist of the measured height of water at each mean solar hour, without reference to high water. The problem consists in the expression of the height of water as the sum of a series of harmonic terms, the argument of each term being derived from theoretical considerations. The method was inaugurated by Sir William Thomson about fifteen years ago, and it was carried out by a previous committee of the Association, which presented its last Report in 1876. Since that date the use of the method has received a wide extension through the action of the Indian Government, who have constituted a Tidal Department of the Great Trigonometrical Survey, with an office at Poona. The Department is ably directed by Major A. W. Baird, R.E., and with the co-operation of Mr. Roberts in England, issues yearly tables of tidal predictions for all the principal Indian ports.

The Report of 1876 left the subject avowedly incomplete, and the great mass of tidal results have accordingly been left in a condition which renders them inapplicable for theoretical discussion without a great deal of preliminary arithmetical work. The Report of the committee of 1882, drawn up by Prof. G. H. Darwin, was presented at the late meeting of the Association. It occupies sixty-eight pages, and is intended to serve as a manual for the use of the harmonic method. The theoretical part of the Report contains a development of the equilibrium theory of tides, from which is derived a schedule of astronomical tides for which the analysis is to be carried out. The shallow-water and meteorological tides are also discussed.

In consequence of the variability of the inclination of lunar orbit to the equator the heights of the various purely lunar tides are variable from year to year. Instructions are given in the Report for the reduction of each tide to a mean value, and for the correction of the phase according to the longitude of the Moon's node. It is the absence of these reductions which renders the existing tidal results at present unfit for theoretical discussion.

Prof. Darwin has been in constant communication with the India Office during the past two years, and Major Baird has undertaken the arduous task of reducing all the old results according to the system recommended in the Report.

Until now no instructions existed in print for conducting the laborious arithmetical operations required in the reductions; but this want is made good by the latter part of the Report, which explains the arrangement of the analysis, and gives samples of the blank forms for computation, drawn up by Mr.

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Roberts. It is recommended that the old plan of operation shall be followed in the main, but considerable modifications are suggested in the treatment of the tides of long period. The last section contains tables of certain functions of the longitude of the Moon's node which are required for the reduction of the lunar tides to mean values. These tables were computed at Poona under the superintendence of Major Baird. It has not hitherto been possible for anyone, excepting the officials of the Indian Government, to carry out the harmonic treatment of tides, because the forms for computation were only privately printed for the India Office, and were totally inaccessible to the public. Prof. Darwin intends to publish an edition of computation forms. It is probable that, through the exertions of General Strachey, permission will be obtained to have the forms reproduced by photozincography, at the office of the Ordnance Survey. Each set of forms consists of a book of over 100 pages of the size known as demy.

It is to be hoped that some of our English harbour boards, and of our colonies, may thus be induced to pay attention to tidal observation, for we are still very far from possessing adequate knowledge of the tides of the world.

The Figure of the Earth.

Prof. G. H. Darwin has also contributed to the *Proceedings* of the Royal Society a short paper giving a solution of the problem of the figure of the Earth, with a different law of internal density from that adopted by Legendre and Laplace.

He shows that this solution is capable of reconciling the observed value of the Precessional Constant with the observed ellipticity of the surface, equally well with Laplace's solution. Two special cases of the proposed solution correspond with the cases of constant modulus of compressibility, and of modulus varying as the first power of the density. In Laplace's solution the modulus varies as the square of the density.

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Papers read before the Society from February 1883 *to February* 1884.

1883.

Mar. 9. Observations of Comet *b* 1882, made with the transit circle of the Royal Observatory, Greenwich. Astronomer Royal.

Observations of the partial solar eclipse 1882, November 10. J. Tebbutt.

Observations of occultations of stars by the Moon, of phenomena of *Jupiter's* satellites, and of the eclipse of the Sun 1882, May 16, made at the Royal Observatory, Greenwich. Astronomer Royal.

Observations of the transit of *Venus* 1882, December 6, made at Cambridge, Mass., U.S.A. E. F. Sawyer.

The proper motions of telescopic stars. Prof. R. Grant.

On the observations of the transit of *Venus* 1882, December 5–6, made at the Lick Observatory, Mount Hamilton, California. Prof. D. P. Todd.

Observations of the transit of *Venus* 1882, December 5–6, made at the Observatory, Kingston, Canada. Prof. J. Williamson.

The variability of β *Cygni* and 63 *Cygni*. Rev. T. E. Espin.

Notes on some of Schjellerup's identifications of Al Sufi's stars. J. E. Gore.

On the account of William Ball's observations of *Saturn* in the *Philosophical Transactions*. H. T. Vivian.

Note on observations of *Mercury*. W. F. Denning.

Observations of occultations of stars by the Moon and of phenomena of *Jupiter's* satellites, made at Stonyhurst in 1882. Rev. S. J. Perry.

Note on Prof. Newcomb's remarks on the Windsor observations of the transit of *Venus* in 1874. J. Tebbutt.

Note on a photograph of the great nebula in *Orion*, and some new stars near θ *Orionis*. A. A. Common.

On the magnifying power of a telescope. J. M. Schaeberle.

Notes upon certain doubtful star-places. Prof. T. H. Safford.

On the relative motion of the components of 6 *p. Eridani*. A. M. W. Downing.

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Observations of comet *a* 1883, made with the Sheepshanks equatorial at the Royal Observatory, Greenwich. Astronomer Royal.

On the visibility of the dark side of *Venus*. Prof. C. V. Zenger.

Note, accompanying drawings of the Great Comet (*b*) 1882, made at the Observatory, Arcetri, Florence. W. Tempel.

Notes on the transit of *Venus* 1882, December 6. Rev. F. Howlett.

Physical observations of *Jupiter*. W. F. Denning.

Sur les observations du passage de *Vénus*, 1882, Décembre 6, faites à l'Observatoire, Moncalieri. Padre F. Denza.

April 13. Spectroscopic observations of Comet Swift 1883. Dr. N. de Konkoly.

On the position of λ *Ursæ Minoris*. John Tatlock.

Notes on the Great Comet (*b*) 1882. E. E. Markwick.

Note on some criticisms made by Mr. Stone on the methods available for determining the solar parallax. David Gill.

Note on the nucleus of the Great Comet (*b*) 1882, (extract from a letter to Mr. Knobel). David Gill.

On Mr. Finlay's pre-perihelion observations of the Great Comet (*b*) 1882. David Gill.

On the computation of the eccentric anomaly, equation of the centre, and radius vector of a planet, in terms of the mean anomaly and eccentricity. Dr. J. Morrison.

Note on the eclipse of *Jupiter's* fourth satellite on April 4, 1883. Capt. W. Noble.

Observations made at the United States Naval Observatory, Washington. Prof. A. Hall.

Observations of Comet *a* 1883, made at the Royal Observatory, Greenwich. Astronomer Royal.

May 11. Measures of the companion to *Sirius*. S. W. Burnham.

Observations of the companion of *Sirius*, made at the Dearborn Observatory, Chicago. Prof. G. W. Hough.

Post-perihelion observations of the Great Comet (*b*) 1882. John Tebbutt.

Astrophysical observations made during the year 1882 at the Herény Observatory, Hungary. Eugen von Gothard.

Physical observations of *Jupiter* and *Mars*, made during the year 1882, at the Herény Observatory, Hungary. Alexander von Gothard.

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Observations of Comet *c* (Schaeberle), made at the Cambridge Observatory with the Northumberland equatoreal and square bar micrometer. Prof. J. C. Adams.

Observations of Comet *a* 1883 (Brooks–Swift), made at the Cambridge Observatory with the Northumberland equatorial and square bar micrometer. Prof. J. C. Adams.

Note on Dr. Morrison's paper on the computation of the eccentric anomaly, &c. Prof. J. C. Adams.

Observations of Sun-spots from May 1877 to February 1883. E. E. Markwick.

On Hell's alleged falsification of his observations of the transit of *Venus* in 1769. Prof. S. Newcomb.

On the solar motion in space. W. E. Plummer.

An explanation of the principal cause of the large errors at present existing between the positions of the Moon deduced from Hansen's Tables and observation; and the cause of an apparent increase in the secular acceleration in the Moon's mean motion required by the tables, or of an apparent change in the time of the Earth's rotation. E. J. Stone.

Note on photographs of κ *Crucis* and of the nebula in *Orion* (extract from a letter to Mr. Stone). R. L. J. Ellery.

Photometric determination of the brightness of the brighter stars north of the equator. Prof. C. Pritchard.

On the orbit of γ *Coronæ Australis*. A. M. W. Downing.

Note with respect to the shadow of the planet *Jupiter*. A. C. Ranyard.

The nucleus of Comet *b* 1882. A. A. Common.

June 8. The annular solar eclipse of 1836, May 15. Alex. Brown.

Preliminary account of a telegraphic determination of the longitude of the Royal Observatory, Cape of Good Hope. David Gill.

Determinations of longitudes on the east coast of Africa. W. H. Finlay.

Note on the chromosphere. Rev. S. J. Perry.

Phenomena of *Jupiter's* satellites observed at Mr E. Crossley's Observatory, Bermerside, Halifax, during the opposition of 1882–3. J. Gledhill.

Observations of U *Monocerotis*, and LL 14551, with a new photometer. Rev. T. E. Espin.

Observations of occultations of stars by the Moon, and of phenomena of *Jupiter's* satellites, made at the Radcliffe Observatory, Oxford, 1877–78, and 1882–83. E. J. Stone.

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Elliptic elements of Comet *b* 1882. John Tatlock.

Ephemeris of the satellite of *Neptune*, 1883–84. A. Marth.

Ephemerides of the satellites of *Saturn*, 1883–84. A Marth.

Ephemeris for physical observations of *Jupiter*, 1883–84. A. Marth.

Postscript to note on M. Loewy's communication referring to an apparatus for the determination of flexures. A. Marth.

Note on Delaunay's expression for the Moon's parallax. Prof. J. C. Adams.

Nov. 9. On the change in the adopted length of the tabular "mean solar day," which takes place with every change in the adopted value of the Sun's mean sidereal motion. E. J. Stone.

Ephemeris for physical observations of *Mars*, 1883–84. A. Marth.

Measures of southern double stars. E. B. Powell.

On humidity as a cause of variation of rate in chronometers. Major-General J. F. Tennant.

Note on the mass of *Saturn*. Prof. A. Hall.

Note on some observations of the transits of *Mercury* and *Venus* made in Australia by Mr. Russell and Mr. Tebbutt. Prof. S. Newcomb.

Observations of the variable star R *Carinæ*, from May 1880 to September 1883. John Tebbutt.

Elliptic elements of Comet 1882 I. (Wells). F. J. Parsons.

Jupiter without visible satellites. Rev. S. J. Johnson.

Disappearance of *Jupiter's* satellites. Wentworth Erck.

Showers of large meteors. W. F. Denning.

On the evidence afforded by the Greenwich Lunar Observations 1862–1881 of a change in the adopted unit of time in 1864. E. J. Stone.

Observations of Comet *b* (Pons-Brooks) made at the Royal Observatory, Greenwich. Astronomer Royal.

Note on the Greenwich mean tabular errors of R.A. of the Sun. Edwin Dunkin.

Dec. 14. Ephemeris for finding the positions of the satellites of *Mars* 1883–84. A Marth.

Ephemeris for finding the positions of the satellites of *Uranus*, 1884. A. Marth.

On the change in the adopted unit of time. Major-General J. F. Tennant.

Rotation period of *Jupiter*. W. F. Denning.

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On the supposed unreliability of the Greenwich Observations of the Sun in mean results. E. J. Stone.

On the supposed possible independence of Bessel's and Le Verrier's expressions for the mean longitude of the Sun, subject to the conditions imposed upon them by their use in practical astronomy in the determination of mean solar time. E. J. Stone.

Note on a narrow belt on the planet *Saturn*. A. C. Ranyard.

Note accompanying drawing of the Great Comet (*b*) 1882 (extract from a letter to Mr. Stone). General G. H. Willis.

On the change in the unit of time implied in the substitution of Hansen's for Bessel's expression for the longitude of the mean Sun. Prof. A. Cayley.

Observations of Comet *b* 1883 (Pons-Brooks) made at the Royal Observatory, Greenwich. Astronomer Royal.

On the spectrum of Comet *b* 1883 (Pons-Brooks) observed at the Royal Observatory, Greenwich. Astronomer Royal.

The orbit of the Great Comet (*b*) 1882. Dr. J. Morrison.

Note on the recent twilight glow. W. Mattieu Williams.

Remarks on Mr. Stone's explanation of the large and increasing errors of Hansen's Lunar Tables by means of a supposed change in the unit of mean solar time. Prof. J. C. Adams.

Observations of Pons's Comet made at the United States Naval Observatory with the 9·6-inch equatorial. W. T. Sampson and E. Frisby.

1884. Jan. 11. Observations of Comet III. 1882 (Barnard). W. H. Finlay.

The meteor shower of Pons's Comet. W. F. Denning.

On the latitude of the Observatory, Windsor, New South Wales. John Tebbutt.

Observations of occultations of stars by the Moon and of phenomena of *Jupiter's* satellites made at the Royal Observatory, Greenwich, in the year 1883. Astronomer Royal.

Suggestion for an observing seat for transit instruments. Richard Inwards.

Note giving a numerical illustration of the effects of a percentage change in the Sun's mean motion in longitude. E. J. Stone.

The belts of *Saturn*. Henry Pratt.

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Spectroscopic results for the motions of stars in the line of sight obtained at the Royal Observatory, Greenwich, in the year 1883. No. VII. Astronomer Royal.

Heliometer determinations of stellar parallax in the Southern hemisphere. David Gill and W. L. Elkin.

Elements of the orbit of Comet *a* 1883 (Barnard's). Robert Bryant.

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List of Public Institutions and of Persons who have contributed to the Library, &c. since the last Anniversary.

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Berlin, International Geodesic Association.
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Genoa, Society of Literature and Science.
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Ottawa, Canadian Meteorological Office.
Paris, Academy of Sciences.
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Paris, Philomathic Society of France.
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St. Petersburg, Imperial Academy of Sciences.
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Washington, Bureau of Navigation.
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Washington, United States Coast and Geodetic Survey.
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Zurich Central Meteorological Institution.
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Editors of the American Journal of Mathematics.
Editor of the Analyst.
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ADDRESS

Delivered by the President on Presenting the Gold Medal of the Society to Mr. Common.

GENTLEMEN,—

Your Council have awarded the Gold Medal to Mr. A. A. Common for his photographs of celestial bodies.

In this award the Council have been less influenced by originality in the methods adopted than by the great practical success which has attended Mr. Common's efforts in a most important and interesting field of astronomical research.

It appears that in 1874 Mr. Common was in possession of a five-and-a-half-inch Refractor, and was even then engaged in astronomical photography. He soon, however, found it desirable to improve his instrumental means, and provided himself with two disks of glass of seventeen inches diameter, with the intention of grinding his own mirrors, and mounting a Reflector of this size. But, fortunately perhaps for our science, Mr. Common abandoned this idea, and supplied himself with an eighteen-inch mirror by Calver. The mounting for this mirror was designed by himself and executed under his direct personal superintendence, and he was able to commence work with this powerful instrument in 1877. But even this instrument appeared insufficient to meet Mr. Common's requirements, and he soon gave an order to Mr. Calver for a mirror of three feet diameter. The first disk of this size burst into fragments under the tool, but a second disk was successfully worked, and the mirror mounted in July 1879. A description of this instrument is given in the *Memoirs*, vol. xlvi. The mounting, which is quite a departure from the usual forms, shows in every direction great engineering skill, guided by the experience gained in the use of the smaller instruments of the actual requirements for successful astronomical work. The method of relieving the friction of the moving parts of the mounting by floating them in mercury, and the special adjustments for the use of the instrument for photographic purposes, are deserving of attention. This powerful instrument has been used in the observation of the fainter satellites of *Saturn* and of *Mars*, and the discovery of many new faint nebulæ, but has been more especially devoted to photographic work. The first attempt at a photograph of the nebula of *Orion* was made on 1880 Jan. 20. The result was a failure. The stars were seen as

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lines, and the nebula proper presented merely a faint stain upon the plate; but such failures only suggested the necessity of improved clock-driving and the use of more sensitive plates. Thus availing himself of every increase of the sensitiveness of the prepared photographic plates and continually improving the control of the driving-clock, Mr. Common was able, on 1881 June 24, to obtain a photograph of the Comet *b*, which is probably the earliest successful photograph of any comet (although a photograph of the comet was obtained on the same night by Dr. Draper); whilst on 1882 March 17, a photograph of the nebula of *Orion* was obtained, which was exhibited at the May meeting of the Society, and excited the admiration of all the astronomers who had an opportunity of inspecting it.

But still the clock-work admitted of some further improvements. It was possible to place a wire to afford a visible point of reference of the fixed direction of the instrument relatively to the stars, and thus to allow of a great extension of the time of exposure of the plates without destroying definition.

These practical improvements rendered it possible to secure the splendid photograph of the nebula which was presented to the Society in March 1883. This photograph was taken on 1883 Jan. 30.

The work has been continued, and to allow for the effects of different exposures and the sensitiveness of the plates to light of different degrees of refrangibility, a long series of experiments have been carried out with exposures increasing from 1^m to 90^m. It appears that even with the very long exposures, more details are brought out by every increase in the length of exposure, and that the extreme limit of useful exposure has not even been reached at $1^h\ 30^m$.

With the comparatively short exposure of thirty-seven minutes, stars such as Lassell's *a* and *b* mentioned in Holden's Monograph of the central parts of the nebula in *Orion* are distinctly shown, as well as some of the faint stars discussed by Mr. Common (*Monthly Notices*, vol. xliii. p. 255); while details of the nebula itself, not represented in the best drawings that have occupied so much time and labour, and which indeed the eye can hardly discern and the hand can never properly represent, are clearly shown in the photographs. The success of these long exposures with this powerful instrument has opened out a new field of research by which the accumulating effect of the light of faint stars, too faint even for observation to the eye, have been registered upon the photographic plate. It is indeed difficult to over-estimate the interest with which the results which Mr. Common may obtain in this direction will be watched by astronomers.

In addition to the work mentioned, Mr. Common has obtained some beautiful photographs of other nebulæ and of the planets *Jupiter* and *Saturn*; and has recently applied himself with success to obtaining photographic star maps to stars of the

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eleventh magnitude, a field in which Professor Pickering has already made some progress.

Such, gentlemen, is a bare record of the steps which have led Mr. Common to success, and the justification of the Council in awarding to him the medal of the Society. The lesson taught is not a new one. The records of our Society are rich in the labours of our amateur astronomers. The amateur who can provide himself with sufficient instrumental means for original research need fear no professional rivalry; untrammelled by the necessity of continuing observations whose value largely depends on their continuity, having the power of taking up any subject he pleases, pursuing it so long as he believes in the possibility of success, without fear or responsibility of charges of wasted time and wasted means, he possesses advantages which are priceless in the tentative and experimental stages of any work.

It is in work of this class that the most striking advances in our science must be expected; and such work will most certainly repay, by the gratification of personal success, the efforts of those who devote themselves to original work in our science, and the field of research presented is absolutely boundless.

Mr. Common,

It is with the greatest pleasure that I place in your hands this medal as a recognition on the part of the Royal Astronomical Society of the great value of your contributions to the advancement of our science.

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The meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLIV. MARCH 14, 1884. No. 5.

EDWIN DUNKIN, F.R.S., President, in the Chair.

John T. Stevenson, Auckland, New Zealand, was balloted for and duly elected a Fellow of the Society.

On the Necessary Distinction in Practical Astronomy between the True Mean Solar Day and the Mean Day adopted, from time to time, in the Construction of our Astronomical Tables, and in the Comparison of these Tables with Observations. By E. J. Stone, M.A., F.R.S.

The papers which have appeared in connection with this question in the *Monthly Notices* for 1883, December, and 1884, January, show that the physical conditions of the problem are most imperfectly understood. We certainly have not two independent mean Suns, and therefore two independent real Suns, and therefore two independent positions of the observer at any given time available in practical astronomy to satisfy any analytical requirements; nor can we measure motion correctly in terms of a time which is itself a fallible quantity, whose errors increase as the time increases; and it is equally impossible for us, so long as we are ignorant of the value of the Sun's mean motion in longitude in the true mean solar day, to determine times from the motion of the meridian *alone* expressed in terms of a unit whose length itself depends upon the *relative* motions of the meridian over that of the Sun in mean longitude; for the two motions are perfectly independent. We have no doubt attempted to do this; and theoretical astronomers have supposed that we were really using the *true* mean solar day as our unit; and it is because we have attempted to do what is physically impossible, and have directly introduced into our

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determinations of time important conditional equations which would be true if we *were* using the true mean solar day as our unit, but which are not true in practice, that we have introduced discordances between our theories and observations which have led to the necessity of such hypotheses as "changes in the time of the rotation of the Earth on its axis," or "unknown physical actions," and have wasted the energies of some of our best men and squandered thousands of pounds.

It is absolutely impossible for us to determine motions in terms of a variable t which is not quantitatively referred to some continuous motion which is itself measurable, or to some such difference between motions; and the equation which connects the standard motion and the variable t, and from which t is determined, is, of necessity, subject to the conditions of its use in practice, *an exact equation*. It is this equation, subject to its use in practice, and not any theoretical views of our own, which fixes the scale upon which our times are measured, or the unit of time which is adopted in practical astronomy. But the observer's position depends upon two perfectly independent motions—the motion of the Earth about its centre of gravity and the motion of that centre of gravity itself. If the symbols ω and n denote the motion of the Earth about its axis, and the mean motion of the centre of gravity of the Earth in longitude respectively, then, before we can compare our theories of the motions of the centres of gravity of the Earth, Moon, and planets with observations, or determine the constants involved in our theoretical expressions, we must establish some relationship between ω and n; but, since the motions of ω and n are perfectly independent of each other, we can only establish such a relationship indirectly through T, some interval of time which we adopt as our unit. With every variation of T, ω and n will both vary, but so long as the symbols represent the physical quantities which I have indicated, and when they no longer indicate these physical quantities, we have no concern with them, the variations must satisfy the equations

$$(1)\quad \frac{\delta\omega}{\omega} = \frac{\delta n}{n} = \frac{\delta T}{T}.$$

If we will introduce in our practical determinations of mean times any conditional equations depending upon the length of the *true* mean solar day, or any other similar physical conditions, we must employ an *exact equation* between ω, n, t, or we shall necessarily introduce secular separations between the two motions which fix the observer's position; but if t denote the length of the true mean solar day expressed in terms of any unit T, we have identically, from the definition of the mean solar day,

$$(2)\quad \omega = \frac{360 \times 60 \times 60}{t} + n.$$

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And if the symbol T_o denote the length of the true mean solar day, we shall have

$$t \,.\, T = T_o,$$

and therefore

$$(3)\quad \omega = 360 \times 60 \times 60 \times \frac{T}{T_o} + n.$$

This equation (3) is true for all corresponding values of ω, T, and n, and is exact, but differentiating we have

$$\delta\omega = 360 \times 60 \times 60 \times \frac{\delta T}{T_o} + \delta n,$$

or since

$$\frac{\delta T}{T} = \frac{\delta n}{n},$$

we have, if $T = T_o$ nearly,

$$\delta\omega = 360 \times 60 \times 60 \times \frac{\delta n}{n} + \delta n.$$

This equation establishes the only possible law of variation between ω and n when ω and n denote the physical quantities of our problem. If, therefore, n_o is the true value of the mean motion in longitude of the centre of gravity of the Earth in the true mean solar day T_o, we have

$$\omega_o = 360 \times 60 \times 60 + n_o;$$

but, when we adopt n instead of n_o, the true expression is not

$$\omega = 360 \times 60 \times 60 + n,$$

as astronomers have hitherto assumed, but

$$\omega = 360 \times 60 \times 60 + n_o + 360 \times 60 \times 60 \frac{n - n_o}{n_o} + n - n_o.$$

or

$$(4)\quad \omega = 360 \times 60 \times 60 + n + 360 \times 60 \times 60 \frac{n - n_o}{n_o},$$

This equation is exact. The omission of the term

$$360 \,.\, 60 \,.\, 60 \,.\, \frac{n - n_o}{n_o},$$

would at once, with times determined with the erroneous value of ω, introduce secular errors into the theoretical positions of the Sun, Moon, and planets. If, therefore, s denotes the sidereal time, we have

$$s = a + 24^h \,.\, t_o + \frac{n_o}{15} \,.\, t_c,$$

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if t_0 is the true mean solar time; but we have with equal exactness

$$s = a + \left(1 + \frac{n - n_0}{n^0}\right) . t + (n_0 + n - n_0) . t,$$

or

$$(5)\quad s = a + \left(24^h + n + 24^h \frac{n - n_0}{n_0}\right) . t,$$

when we adopt n for the Sun's mean motion. The only difference is that our time now is not *true mean solar time*, but mean time referred to some definite unit T.

If, therefore, we attempt from a given observed value of s to infer the true sidereal interval from mean noon by the formula usually adopted,

$$s = (a + nr),$$

where r is the number of complete days from epoch, we commit the error

$$-24^h . \frac{n - n_0}{n_0} . r.$$

But if we are using $n + \delta n$ instead of n we commit the error

$$-24^h \left(\frac{n + \delta n - n_0}{n_0}\right) . r.$$

The difference between these errors is a determinate quantity,

$$-24^h \frac{\delta n}{n_0} . r.$$

The question is, Can we neglect this difference? The answer is emphatically, No! I have proved this by a direct numerical calculation. It accounts for the existing large discordances between the lunar tables and observations, and such terms cannot therefore be neglected as insensible.

If we start from the given sidereal time of observation, as we do in practice, these results show that, instead of taking out our tabular places for the same noon before and after the change from n to $n + \delta n$, we must correct all our tabular places for an error of time

$$24^h \frac{\delta n}{n} . r,$$

in order that they may correspond to the new mean noon as they did correspond to the old mean noon. The difficulty which appears to be felt in this matter is probably due to overlooking the fact that if we commit such systematic errors in referring our sidereal times to our mean times, as I have proved we do commit so long as n_0 is unknown, we determine in practice not the true mean solar times, but other times; and we express, as the only times available to us, all our mean motions

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in terms of these times, and not in terms of the true mean solar times as we have hitherto supposed. A change in the adopted value of n, therefore, necessitates a redetermination of all our mean motions, or a correction to the times such as I have applied; for when we determine our times directly from the motion of the meridian with any adopted value of ω, *all errors in our time determinations are thrown directly upon the theoretical expressions for the motions of the centres of gravity.*

This explains why Prof. Adams, in attempting to show that no errors have been made in these questions of tabular times, is driven to assert that we have nothing to do with the Right Ascension of the meridian at mean noon; and that my views must be wrong because the adopted tabular expressions for the motions of the centres of gravity are made to represent the same motions in the same absolute times before and after any arbitrary changes which we may be pleased to adopt in our time determinations. The way in which these small errors accumulate is no doubt difficult to get a firm grasp of. And perhaps the point may be put somewhat more simply as follows.

If we adopt a value n_1 for n, then the unit of time is certainly fixed and determinate. Let this value be T_1, and let ω_1 be the value of ω expressed in terms of T_1, and t_1 the length of the true mean solar day, then clearly

$$t_1 = \frac{360^\circ}{\omega_1 - n_1}.$$

And of n_2, T_2, ω_2, and t_2 are other corresponding values,

$$t_2 = \frac{360^\circ}{\omega_2 - n_2}.$$

and

$$\frac{t_1}{t_2} = \frac{\omega_2 - n_2}{\omega_1 - n_1}.$$

But since these equations are exact, we have

$$\frac{\omega_2 - n_2}{\omega_1 - n_1} = \frac{T_2}{T_1} = \frac{n_2}{n_1}.$$

We have, therefore,

$$\frac{t_1}{t_2} = \frac{n_2}{n_1},$$

or

$$\frac{t_1 - t_2}{t_2} = \frac{n_2 - n_1}{n_1}.$$

It is, therefore, perfectly impossible for us to use the true mean solar day as our unit when we are continually changing the adopted values of n. But the question practically under consideration is, What will the errors of the false assumption amount to? The equation

$$\frac{t_1 - t_2}{t_2} = \frac{\delta n}{n}$$

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answers the question. We shall make an error of

$$\frac{\delta n}{n} \times \text{days measured.}$$

The change thus made in 1864 accounts for the large increase of the discordances between Hansen's tables of the errors and observations, and the errors of the false assumption, therefore, cannot be neglected. But it may be said that astronomers have proved that the changes in the units of time due to changes from n to $n+\delta n$ in the adopted values of the Sun's mean motion in longitude are not in the ratio of $n : n+\delta n$, as assumed, but in the ratio of $\omega : \omega+\delta n$. Astronomers have certainly proved nothing of the kind, and it is perfectly impossible that any such result can be true when ω and n represent the physical quantities which I have made them represent. Prof. Cayley has proved that if we have two Suns—or two Earths, rather—moving with mean motions in longitude of n and $n+\delta n$ respectively in the adopted unit of time, then the mean solar days would differ by nearly

$$\frac{\delta n}{\omega} \times \text{length of a day.}$$

Prof. Cayley's result is of course perfectly correct on the assumptions made; but in practical astronomy the observer's position is fixed through the n and the ω, and n expresses the mean motion of the centre of gravity of the Earth in longitude. The n and $n+\delta n$, therefore, refer to the motion of the same physical point, and not to two independent moving points, as Prof. Cayley supposes in his problem. The result given by Prof. Cayley, therefore, does not prove that

$$\frac{\delta T}{T} \text{ is not } = \frac{\delta n}{n}$$

when the symbols have the physical meanings which I have assigned to them. As a matter of fact, Prof. Cayley obtains my result when restricted to a single moving point.

Prof. Adams has also shown that, subject to certain conditions,

$$\frac{\delta T}{T} \text{ is equal to } \frac{\delta n}{\omega}.$$

And this result is equally true on the assumptions made, but they are not those of the physical problem under consideration, as we can easily show. If we attempt to establish a relation between ω and n by successive approximations through ω, starting from $\omega = 360°$ in the sidereal day S, we can do this quite accurately provided we introduce no physical conditions which the symbols ω, n, and T are made to satisfy in our practical determinations of the time during the process of approximation. For, putting

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$\omega = 360° + n$ and $360° + n + \delta n$ respectively, we have for the sidereal time respectively

$$s = a + \left(24^h + \frac{n}{15}\right)t$$

and

$$s = a + \left(24^h + \frac{n+\delta n}{15}\right).t'$$

and therefore

$$= t' \left(1 + \frac{\delta n}{360° + n}\right).$$

But this result is utterly inconsistent with the assumption which is always made in practical astronomy that the true mean solar day is always used as our unit, and that we can therefore neglect the difference between t and t' in the large terms $24^h t$ and $24^h t'$, and thus write

$$24^h . t = 24^h . t'$$

whenever t' is an integer; this requires that

$$s = a + \frac{nt}{15} = a + \frac{(n+\delta n)t'}{15},$$

and therefore instead of

$$t = \left(1 + \frac{\delta n}{\omega}\right).t',$$

$$t = \left(1 + \frac{\delta n}{n}\right).t',$$

to satisfy the necessary conditions.

Prof. Adams's result is therefore not true, as we use the expressions

$$360 \times 60 \times 60 + n$$

and

$$360 \times 60 \times 60 + n + \delta n$$

in our practical determinations of time.

This point is rendered clearer by returning to the accurate equation which I have proved,

$$s = a + \left(\frac{360 \times 60 \times 60}{15} \cdot \frac{T}{T_0} + \frac{n}{15}\right)t.$$

For since $t\,T = t_0\,T_0$, identically, if t_0 denote the true mean solar time, we may write this equation under the form

$$s = a + 24^h\, t_0 + \frac{n}{15}.t.$$

If we change n into $n + \delta n$, since

$$(T + \delta T)t_1 = t_0 T_0,$$

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we have

$$s = a + 24^h t_0 + \frac{n+\delta n}{15} . t$$

These equations show at once that if we always suppose the time measured in true mean solar time in the principal term 24^h, we simply throw the adjustment of the mean times to the given sidereal times upon the small terms. If we determine our mean solar time, firstly, from the equation

$$s = 24^h . t_0 + \frac{n}{15} . t,$$

secondly, from the equation

$$s = 24^h . t_0 + \frac{n+\delta n}{15} . t',$$

then the times must differ in the proportion of the units $n : n+\delta n$. And if we write these expressions

$$s = a + 24^h . t + \frac{n}{15} . t$$

and

$$s = a + 24^h . t + \frac{n+\delta n}{15} . t,$$

and simply assume that the mean noons are the same in both cases, so that we can put $t = r =$ number of true mean solar days at true mean noon, which comes to the same thing as writing

$$t = t_0 \text{ in the principal term,}$$

we must find the values of t as before expressed in terms of units in the proportion of $n : n+\delta n$.

All I have done, therefore, is to simply transfer Le Verrier's expression so as to make it give identical results with those which would have been obtained by the direct use of Bessel's expression unaltered since 1864.

This leads to an important remark and question of Professor Cayley's in the January number of the *Notices* to which I have not yet alluded. Professor Cayley says (p. 85): "Mr. Stone, as I understand him, contends that for the same instant of time the two points (i.e. really the observer's position) are one and the same point." If so, it seems to me that Hansen's formula (or Le Verrier's) must be a mere analytical transformation of Bessel's, and the question would arise, Why was such a change made?" The change made in our expression for the sidereal times *was quite unnecessary*. It was done no doubt because it was supposed that the value of the Sun's mean motion deduced from a discussion of the observations in terms of the adopted unit of time was more accurate than that adopted by Bessel, and it was not known apparently that by adopting this value in the expression for the sidereal time in terms of the solar time we

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should necessarily change the adopted unit. But, whatever may have been the reason for the change, there is only one way of estimating its effects, and that is, to transfer the variable t' of Le Verrier (or Hansen), as Professor Cayley practically admits I have done, into that of the t of Bessel. This, at all events, gives correctly the effects of the change upon existing tables. What we want is to separate the discordances between Hansen's tables and observations into two parts—one due to our arbitrary action in 1864, and the other due to errors in the tables themselves. As some serious misconceptions appear to exist upon the point, it should be known that Le Verrier deduced the correction δn to the adopted value n from a discussion of the observations referred to the meridian with the condition

$$s = a + \left(24^h + \frac{n}{15}\right)t + \text{ \&c.};$$

and therefore the value $n + \delta n$, so far as the correction δn was really required, was only true when this relationship existed between s and t; when, therefore, after the construction of the tables, we adopted

$$s = a + \left(24^h + \frac{n + \delta n}{15}\right) . t',$$

and assumed at every mean noon, instead of

$$sn = a + \frac{n}{15} . t,$$

that

$$sn = a + \frac{n + \delta n}{15} . t',$$

we actually introduced the error

$$(n + \delta n)t' - nt,$$

or, if t' is supposed to be equal to t, the error

$$\delta n t,$$

or, practically, sensibly doubled the correction δn which Le Verrier thought necessary to reconcile theory and observation with the old unit of time.

With respect to the inexactness of such equations as those connecting s and t: they are used in two ways, in both of which they must be exact; we determine from such equations the mean time from a given sidereal time, or we eliminate by the aid of this equation the variable t, and thus find the tabular positions of the Sun, Moon, and planets on the meridian for direct comparison with our observations.

The following is perhaps the simplest form in which the proof of the change can be put.

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If

$$(\alpha)\quad s = a + \left(24^h + \frac{n}{15}\right)t$$

is Bessel's expression, I *can* certainly transfer t into t' time as follows. Let

$$nt = (n + \delta n)t',$$

then

$$(\beta)\quad s = a + \frac{n+\delta n}{15}.t' + 24^h\left(1 + \frac{\delta n}{n}\right).t',$$

Le Verrier's expression adopted in 1864 was

$$(\gamma)\quad s = a + \frac{n+\delta n}{15}.t' + 24^h.t'.$$

The addition of the term

$$24^h.\frac{\delta n}{n}.t'$$

makes, therefore (γ) identical with (β), and it can, therefore, be re-transferred identically back into (α). This term is, therefore, the necessary correction to make the mean times deduced from Le Verrier's expression identical with those which would have been obtained from the continued use of Bessel's expression. And since the change from Bessel's expression to Le Verrier's in 1864 was an arbitrary act of astronomers, the discordances thus introduced between Hansen's Lunar Tables and observations are the results of our own action, and have nothing whatever to do with physical causes; and what we require to estimate is the *effect of the change*, and this is what I have done.

I am perfectly at a loss to understand the opposition with which my work has been received. It appears to be considered that there is some absolute physical necessity for the use of the true mean solar day as the unit of time in practical astronomy, although such a use is absolutely impossible for the reasons which I have already given.

Questions respecting Mr. Stone's Theory of Changes in the Mean Solar Day. By Prof. Simon Newcomb.

I have refrained from joining in the discussion of Mr. Stone's views respecting the mean solar day found in the *Monthly Notices* of May last for two reasons. One is that the matter seemed too elementary in its character to be the subject of prolonged discussion. The other was a desire to wait until Mr. Stone should give a statement of his views sufficiently explicit to admit of their correctness being tested by practical application. As the discussion has now been going on for the greater part of a year, the first of these reasons has ceased to be operative. At the same time I note with surprise that Mr. Stone still refrains

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from any such statement of his theory as would guide one in making any practical application of it, and therefore ask permission to point out what is wanted. In doing this I do not wish to be considered as taking any ground against his conclusions, but rather as viewing him in the light of a plaintiff who has failed to make a sufficiently explicit statement of his case.

On page 336 of volume xliii. of the *Monthly Notices* Mr. Stone gives a formula for the apparent deviation of the Moon's mean longitude arising from the cause which he claims to act. This expression contains the algebraic symbol t in the product $0''.747\ t$, where it is understood that t is the time in years counted from *some* epoch. But neither here nor in any of his subsequent papers, so far as I have found, does he give any general definition of this epoch. Again, on the next page he gives observed corrections of the Moon's mean longitude. But he does not state what his computed corrections for the same dates would be, and thus does not afford the means of determining, even by induction, the epoch of t.

It is apparently implied that the epoch is that at which Le Verrier's tables were introduced into the British *Nautical Almanac*. But here again we must remark that no statement is made to the effect that the introduction of tables into an Ephemeris constitutes a definable epoch in the application of the tables. The same lack of general definition and explicit statement pervades all of Mr. Stone's statements on the subject which I have been able to find. As it is of great interest to all investigators of the lunar theory to know to what extent their work is to be modified should his views be proved correct, I submit the following questions respecting the application of his theory in the spirit of a seeker after information, and with the earnest hope that either Mr. Stone or someone else who has mastered his theory will answer them:—

I. What is the *general* definition of the epoch of t in the expression already alluded to on page 336?

II. What are the theoretical deviations of the Moon's mean longitude arising respectively from the introduction of Le Verrier's tables into the *Nautical Almanac* in 1864, and of Hansen's tables, the mean motion of which is substantially identical with that of Leverrier into the American *Ephemeris* in 1858?

III. On what principle or by what rules are Professor Airy's conclusions respecting the errors of Damoiseau's lunar tables and my conclusions in § 15 of *Researches on the Motion of the Moon* respecting the corrections to Hansen's tables to be modified?

IV. In lieu of this last statement any general formula or precept showing completely by what numbers and on what principles the necessary modifications are to be made, would be acceptable.

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Remarks on the Value of the Secular Acceleration of the Moon's Motion derived from Observation. By Prof. Simon Newcomb.

In Thomson's and Tait's "Natural Philosophy," second edition, part ii. p. 415, and Appendix G, p. 503, are found discussions by the authors and by Professor Darwin of the earth's tidal retardation as deduced from various values of the Moon's secular acceleration. The statements of the origin and relations of these different values which accompany these discussions seems to me susceptible of some slight reconstruction, which I am induced to attempt from the belief that any incompleteness which may affect them arises from some defect in my own presentation of the subject, to which the authors refer. I remark first that the value 12″·56, though clearly described in one passage as founded on erroneous theory, which it really is, is spoken of elsewhere as representing the ancient eclipses, and therefore to be regarded as a value derived from observations. In reality, however, this value is not compatible with any observational data whatever. It is true that Hansen's tables, into which a value little different from this is introduced, do represent the ancient eclipses of Thales and Larissa. But this happens through Hansen having used an erroneous mean motion of the Moon at the present epoch. The process of determining the acceleration from ancient eclipses consists essentially in carrying the place of the Moon back with its present mean motion to the epochs of the eclipses, and comparing the outstanding difference between the mean longitude thus computed and that given by the eclipse. Now, in consequence of Hansen's addition of an empirical term of long period to represent the Moon's place from 1750 to 1850, his centennial mean motion is about 29″ in error. When this correction is applied, the acceleration is diminished by 1″·25, thus making it 10″·9 or 11″·3 according to the value which is considered as best representing the data in question.

We may then say that 11″ is the value which best represents the ancient solar eclipses referred to. But this does not exhaust the subject. In my discussion in *Researches on the Motion of the Moon* I showed that this value was incompatible with the great mass of astronomical data, especially the Ptolemaic and Arabian eclipses of the Moon. Since this publication new light has been thrown on the question from two quarters. The tendency of this new light is to establish the smaller value as nearer the truth.

I. As favouring the larger value I cited the incompatibility of the smaller value with a certain eclipse of the Moon supposed to have been observed at Babylon in the year B.C. 382. Now Dr. Oppolzer calls attention to the fact that Hipparchus does not describe this eclipse as actually observed at Babylon, but as being employed as if it had been there observed.

II. Celoria's ingenious research into the eclipse of Theon,

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which was total in the Hellespont, seems to show that this was the eclipse of Agathocles; and, if so, the shadow passed north of Sicily. This is compatible only with the smaller value of the acceleration.

If we accept these conclusions or interpretations, nothing is left to support the larger value of the acceleration except the eclipses of Thales and of Larissa, while against it stands an array of evidence which I cannot but regard as many times stronger.

The Parallax of α Tauri. By Prof. Otto Struve.

(*Extract from a Letter to Mr. Gill.*)

With regard to parallaxes the following recent fact will be of some interest for you. During the last few weeks I have been engaged in preparing for publication my second volume of double star measures (vol. x. of the *Observations de Poulkova*). On this occasion I was struck with the remark that about thirty years ago I had collected for *α Tauri* twenty-five measures of each coordinate, and seeing that these measures were favourably distributed in different seasons, I proposed to one of my assistants, M. Shdanow, to investigate if they did not exhibit traces of a sensible parallax. The results of his computations were rather surprising. From the distances he found

$$\pi = +0''.500 \pm 0''.075,$$

and from the positions

$$\pi = +0''.538 \pm 0''.089,$$

the mean being

$$\pi = +0''.516 \pm 0''.057.$$

Such an annual parallax would hardly be expected for a star having so small a proper motion as *α Tauri*, but the agreement of the two values, obtained by two so totally different methods of observation as those of distance and direction, can certainly not leave a doubt as to the existence of a comparatively large parallax, though perhaps, according to the probable error, the real parallax might be somewhat less than that given by my measures.

If you think proper you have my authority to communicate this result to the Royal Astronomical Society.

Pulkova: 1884, *March* 1.

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On an Instance of Change of Personality in Observing Position Angles of Double Stars; and on the Orbit of α Centauri. By A. M. W. Downing, M.A.

In vol. xlvii. of the *Memoirs* of this Society there is a paper by Mr. H. C. Russell containing observations of double stars made at the Sydney Observatory during the years 1870–1881. In this paper will be found a considerable number of observations of *α Centauri*, some of the earlier of which have been used by Dr. Elkin in the determination of the orbit of this binary star contained in his valuable paper, *Ueber die Parallaxe von α Centauri*. But a good many of the Sydney observations made since 1877·5 have not been used in Dr. Elkin's investigation; and these, together with some recent measures by M. Hall, Ellery, and Tebbutt, are available for determining corrections to Elkin's elements in order to obtain an orbit which shall represent the later as well as the earlier observations.

With this object in view the necessary calculations were made, and it was found that, in the mean, the position angles computed from Elkin's elements required corrections of $-7\frac{1}{2}°$ in 1878, and of $-3\frac{1}{4}°$ in 1879–81, to make them agree with the observed position angles. An attempt was then made to modify Elkin's elements so as to make them represent the series of observed angles. After working at the subject for some time, however, I convinced myself that these corrections were erroneous, as I was unable to construct an ellipse which would satisfy the earlier as well as the later measures. As it was the introduction of the Sydney observations made after 1878·0 which caused the great discordance between the computed and observed angles, I determined to exclude them, and then found that the requisite corrections became $-1°·14$ in 1878 and $-1°·26$ in 1879–81. It was evident, then, that the corrections to the assumed elements would be quite small, and I proceeded to find them in the manner now to be described.

From a consideration of the outstanding errors of position angles computed from the assumed elements, the following seven normal places were formed (the angles being measured from the circle of declination passing through the principal star at 1860·0):—

	Epoch.	Normal Angle.	Computed Angle.	Normal Distance.	Computed Distance.
		°	°	"	"
(1)	1827·530	213·34	213·27		
(2)	1835·997	219·34	218·81	16·84	16·69
(3)	1852·177	258·76	258·96	5·50	5·40
(4)	1858·745	335·05	335·12	4·69	4·85
(5)	1875·455	36·61	36·61	5·97	5·92
(6)	1878·380	139·23	139·01	1·96	1·85
(7)	1880·361	187·44	187·70	5·32	5·51

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Seven equations of condition were then formed, by means of the usual formula, for the determination of corrections to T, P, and *e*, and solved by the method of least squares, and from the new values of these elements and Elkin's values of λ, γ, and ϐ, position angles corresponding to the last six epochs were computed, and from the differences between these and the normal values, and from differential coefficients formed by varying λ, γ, and ϐ, equations of condition for determining corrections to these elements were formed, and hence the required corrections found by the method of least squares. The position angles computed from the new elements are given in the column headed "Computed Angle." A correction to Elkin's value of the semi-axis major was then found from the new values of the other elements in conjunction with the normal distances, allowing half weight to the normal distance corresponding to 1878·380. The elements thus found are as follows:—

T	...	...	1875·951
P	...	...	76·222 years
e	...	...	0·51578
λ	...	...	54° 59′·2 (1860·0)
γ	...	...	79 15·2
ϐ	...	...	25 30·5
a	...	...	17″·33

A detailed comparison of places computed from these elements with the observations made since 1870 is given at the end of this paper. This comparison shows that up to the end of 1877 the Sydney measures of position angles agree, within the limits of error of the observations, with those of other observers, but that in 1878 a remarkable change in the manner of observing occurred, which is continued, though in a less degree, in 1880. This change, which makes Mr. Russell's measures in 1878 (of which there are a considerable number) differ by 5 or 6 degrees from the average of other observers, may perhaps be connected with the change of direction of the line joining the components; the *comes* having passed from the n.f. to the s.f. quadrant towards the end of 1877. It is a useful illustration of one of the many difficulties which beset those who are engaged in investigating the orbits of double stars.

The observed position angles or distances enclosed within brackets in the following table have not been used in determining the elements given above: those measured by Mr. Powell because they were not published at the time when the normal places were formed; the others because of their discordance. It is evident that, thanks to Dr. Elkin's able and thorough investigation, we are now in possession of elements of *α Centauri* which will probably require but small correction in order that they may represent observations made in the future. The corrections deduced in the present paper are of course not entitled to much

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weight, as they depend on but few observations; and the subject has been brought forward chiefly with the view of calling attention to the change in the method of observing referred to above.

Epoch.	Observer.	θ_o	$\theta_o - \theta_c$	ρ_o	$\rho_o - \rho_c$
1870·100	Powell	20°45	−0°11	10″24	+0″07
70·608	„	(21·80)	+0·21	(10·09)	−0·48
70·746	Russell	22·32	+0·42	10·46	−0·08
71·050	Powell	(23·01)	+0·52	(9·89)	−0·68
71·310	„	(23·70)	+0·67	(9·80)	−0·59
71·473	Russell	22·91	−0·46	10·23	−0·11
72·468	„	25·33	−0·24	9·73	−0·04
73·331	„	28·07	+0·27	9·50	+0·50
74·150	Ellery	30·50	+0·15	8·00	−0·01
74·468	Russell	30·00	−1·55	7·97	+0·42
74·850	Lindsay	34·17	+0·99		
75·020	Seeliger	34·21	+0·20	6·82	+0·14
76·411	Russell	46·97	+1·52	4·35	+0·27
76·615	Ellery	51·05	+2·55	4·15	+0·47
76·940	„	51·20	−3·87		
77·140	M. Hall	64·40	+3·77	3·30	+0·62
77·248	Ellery	(69·10)	+4·80		
77·510	Russell	72·78	−3·07		
77·556	„	77·25	−1·10	2·11	+0·09
77·580	Gill	80·50	+0·81		
77·593	Russell	81·74	+1·30	1·90	−0·07
77·629	Gill	81·52	−1·08		
77·834	„	97·14	+0·49		
77·888	„	101·04	+0·26		
78·153	Russell	(116·11)	−6·07	1·77	+0·08
78·225	„	(121·66)	−6·11	1·98	+0·26
78·279	„	(127·37)	−4·44	1·77	+0·01
78·380	M. Hall	139·10	+0·22	2·40	+0·55
79·252	Ellery	174·40	+0·83	3·41	+0·14
79·474	Hargrave	(173·55)	−4·17	3·41	−0·30
80·186	Tebbutt	186·70	+0·53	4·97	−0·19
80·351	„	186·40	−1·08	5·31	−0·18
80·444	Russell	(184·98)	−3·18	5·32	−0·36
81·279	Hargrave	(189·88)	−2·81	(5·07)	−2·25
81·545	„	(190·13)	−3·61	7·52	−0·31
1881·655	Tebbutt	193·15	−0·99	7·95	−0·09

Blackheath: 1884, *March* 13.

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Note on the Determination of the Planes of the Orbits of Jupiter's Satellites. By A. Marth.

The values of the nodes and inclinations of the orbits of *Jupiter's* satellites which are derived from observed eclipses are liable to a very sensible amount of uncertainty. The eclipses of longest duration give, indeed, the places of the nodes on *Jupiter's* orbit, while those of shortest duration furnish the inclinations. But as the eclipses which occur near the nodes vary little in duration, the ordinary errors of the observations, and, still more, the usual want of observations of successive eclipses at the proper epochs, leave considerable uncertainty in fixing the places of the nodes; and, in the case of the eclipses of shortest duration, if they occur far from the centre, an additional difficulty arises from the uncertainty of the real extent of the sections of the shadow by which the eclipses are produced.

So long as the observations of eclipses were the only trustworthy means available for finding not only the longitudes of the satellites in their orbits, but also the positions of their orbits, the ensuing uncertainty had to be submitted to as unavoidable. But since superior instruments and micrometers have become available, advantage ought to have been taken and ought to be taken to employ them at the proper times for the purpose of procuring those elements which cannot be derived from eclipse observations with the same degree of accuracy. As the apparent motion of the satellites at the beginning and end of nearly central eclipses amounts, during one minute of time, for

Sat. I., II., III., IV.,

to $0'.36, 0''.29, 0''.23, 0''.17,$

when *Jupiter* is nearest to the Earth, or when *Jupiter* is at its mean distance to

$$0''.27, 0''.22, 0''.17, 0''.13,$$

it is easily seen that for determining the longitudes of the satellites in their orbits, micrometrical measurements are not likely to supersede in accuracy trustworthy observations of the times of eclipses, at least when the satellites do not pass too far from the centre of the shadow cone. But the case is different with regard to the determination of the planes of the orbits, especially the fixing of the nodes.

The most promising means for determining the longitudes of the nodes with accuracy will be found in micrometrical measurements of the distances at which the shadows of the satellites near their mid-transits appear from the centre line which bisects *Jupiter's* disc perpendicular to the axis of rotation, if these measurements are made about the times when the planes of the orbits pass through the Sun. That is the case in the present

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year, and though the opportunity may not be a very favourable one, since the nodal passages occur when *Jupiter* is too near the Sun to allow proper observations to be made, observers will not soon be so favoured as they were in 1849 or at the end of 1860, when, however, the opportunities were neglected, and they ought to make the best of present circumstances.

The inclinations γ and the ascending nodes Γ of the orbits of the four satellites in reference to the plane of *Jupiter's* equator have been given since 1876, at sufficiently close intervals, in my annual Ephemerides for physical observations of *Jupiter*, the nodes being reckoned from O, the point of the descending node of the planet's equator on its orbit. They depend on data derived from Damoiseau's tables of the satellites, and therefore contain the imperfections of the tables affecting the latitudes of the satellites. The following is a continuation of these values for the present and for the next apparition of the planet:—

	Sat. I.		Sat. II.		Sat. III.		Sat. IV.	
	γ_1	Γ_1	γ_2	Γ_2	γ_3	Γ_3	γ_4	Γ_4
1883.	°	°	°	°	°	°	°	°
July 28	0·0108	330·8	0·4814	332·38	0·1634	256·93	0·3180	330·80
Sept. 26	·0108	328·5	·4820	330·43	·1627	256·47	·3175	330·70
Nov. 25	·0108	326·3	·4826	328·49	·1620	256·03	·3171	330·60
1884.								
Jan. 24	·0108	324·1	·4832	326·54	·1613	255·61	·3168	330·50
Mar. 24	·0108	322·0	·4838	324·60	·1607	255·21	·3166	330·39
May 23	·0109	319·8	·4844	322·66	·1601	254·83	·3166	330·29
July 22	·0110	317·7	·4850	320·72	·1595	254·48	·3166	330·20
Sept. 20	·0111	315·7	·4857	318·79	·1589	254·15	·3167	330·11
Nov. 19	·0111	313·7	·4863	316·86	·1582	253·84	·3169	330·04
1885.								
Jan. 18	·0112	311·9	·4870	314·94	·1575	253·55	·3172	329·98
Mar. 19	·0113	310·1	·4876	313·02	·1568	253·28	·3176	329·93
May 18	·0114	308·4	·4883	311·11	·1561	253·02	·3179	329·91
July 17	0·0115	306·7	0·4889	309·20	0·1553	252·77	0·3183	329·90

The angular distances from the centre of the disc at which, heliocentrically, the apparent orbits of the satellites cross the central meridian will be, according to the assumed data, the following:—

	Sat. I.	Sat. II.	Sat	Sat. IV.
	"	"		"
1884, Mar. 24	−1·09	−2·34	−3·52	−5·70
May 23	−0·60	−1·71	−2·26	−3·72
July 22	−0·11	−1·07	−0·99	−1·74
Sept. 20	+0·37	−0·43	+0·25	+0·22
Nov. 19	+0·84	+0·21	+1·48	+2·16
1885, Jan. 18	+1·30	+0·85	+2·69	+4·05
Mar. 19	+1·74	+1·49	+3·86	+5·90

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The conjunction of *Jupiter* with the Sun takes place on August 7. The eclipses of longest duration of the satellites III. and IV. occur on September 4, when *Jupiter* is only 20° distant from the Sun. For testing and correcting the tabular places of the nodes, corresponding observations of the shadows when south and when north of the centre of the disc are required, and observers with superior telescopes and micrometers at their command should endeavour to procure such observations during the latter part of the present and the first half of the next apparition of *Jupiter*.

As regards the determination of the inclinations, it is at present uncertain whether the inclination of the orbit of the *second* satellite can be derived from micrometrical measurements with greater advantage than from eclipses, as the degree of accuracy with which the distances of the shadow can be measured is not yet known. In the case of the *third* satellite, where the number of observed eclipses of shortest duration is much more limited, micrometrical measurements, made at the proper times, will be of essential service for settling the inclination. And in the case of the *fourth* satellite they will be found indispensable if the existing uncertainty is to be effectually removed.

To appreciate this necessity properly, and to get some fair notion of what evidence the observed eclipses of the fourth satellite really furnish for correcting the tabular values of the node and inclination, it may be sufficient, in the absence of an accessible general collection of eclipse observations, to consider the result of those made at Greenwich. The Greenwich eclipse observations, which extend over a longer period than those at any other observatory, have been made with great assiduity, and, I presume, no fair opportunity has been missed; yet, in the course of forty-eight years—from 1836, the year of the publication of Damoiseau's tables, to 1883—the number of eclipses, of which both the beginning and the end have been observed, is only *five*, and of one-sided observations, nine disappearances and sixteen reappearances of the satellite have been secured. None of the five complete observations affords help for correcting the latitude of the satellite. Observations made near the beginning and ending of a cycle of eclipses, when a slight error in latitude shows itself in a telling manner in the observed duration, are very rare. The interesting observation made 1877, August 29, by Todd at Adelaide (*Monthly Notices*, vol. xxxix. p. 16), when the satellite never disappeared, but was reduced to a mere speck of light skirting the margin of the shadow, indicated such an error very strikingly, for the eclipse was, according to the tables, not the first, but the second one of a new cycle, and the tabular duration was seventy-nine minutes. The eclipse of April 4, 1883, the second one of the present cycle of eclipses, showed a similar error, for, according to the observation made with the telescope of the Oxford heliometer by Bellamy (*Monthly Notices*, vol. xliii. p. 446) the satellite disappeared for only six instead of the

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predicted eighty-six minutes, while if the "last seen" and "first seen" really refer to corresponding phases, the predicted time of conjunction is nearly correct.

But important as these two observations will ultimately be for determining the extent of the shadow section, they must be supplemented by corresponding observations of eclipses occurring towards the ends of cycles, in order to allow further inferences to be drawn. The only observation near the end of the last cycle which I have yet seen is a one-sided one, a disappearance observed 1880, May 30, by Tebbutt at Windsor, New South Wales (*Astron. Nachr.*, No. 2,541), from which it may be inferred that the tabular longitude of the node requires a sensible positive correction, as the measurements of Bessel and of Schur had already rendered probable. Observers ought to be on the alert early in 1886 to get complete observations of some of the last eclipses in the present cycle.

Observations of Sun-spot Spectra in 1883. By the Rev. S. J. Perry, F.R.S.

The daily routine work at Stonyhurst Observatory has now for some years included a careful drawing of the solar surface on every available occasion, and also a complete measure of the Sun's chromosphere. Since 1882 a spectroscopic examination of Sun-spots has been added to the previous solar work, but attention has been directed exclusively to the red end of the spectrum, from D to B, as the more refrangible portion is being already carefully studied by others. The following paper is a synopsis of the observations made during the past twelve months, and will, it is hoped, present the results in the most simple form for comparison with the work of other observers. In numbering the lines seen widened or darkened in the spots, the figures published in the British Association Report for 1878 have been followed throughout, and the numbers of both Kirchhoff and Ångström are given for the convenience of reference. The instrument used in these observations is an automatic spectroscope of Browning, in which a dispersion of 2, 4, 6, 8, 10, or 12 prisms of 60° can be readily obtained, and a Christie half-prism has been adapted to the original spectroscope, by which the dispersion previously obtained can be trebled. The figures in the columns headed "widened" and "darkened" will not give averages, as clouds often stop an observer in the middle of his scrutiny, and then only a small proportion of the affected lines may have been noted.

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TABLE I.

Lines between B *and* D *observed in Sun-spot Spectra.*

No.	Kirchhoff.	Ångström	Number of times Widnd.	Darknd.	Elements.	Remarks.
1	592·7	6867·1		2		B.
2	601·2	43 8		3	Sn	
3	641·0	6717·16	1	3	Ca	
4	654·3	6677·6	2	3	Ba	Chromosphere line.
5	659·3	63·1	2	3	Fe	
6	665·7	43·1	2	3		
7	669·5	33·3	2	3		
8	682·8	6593·3	3	5	Bi, Fe	
9	683·1	92·6				
10	685·3	85·9	2			
11	689·8	74·0	2	3	Fe	
12	690·9	71·4	2			
13	692·1	67·9	2			
14	694·1	62·10	1		H	C twice weakened in spot, once reversed.
15	702·1	43·23	2	3	Fe, Ti	
16	711·4	15·80	3	3		
17		6495·12	11		Fe, Ba, I	
18		94·18				
19	723·7	82·79	6		N	
20	726·7	74·85	6			
21	731·7	61·98	1		Ca, Sb, Cd, Cr	
22	740·9	38·35	2		Ca, Cd	
23	756 9	6399·28	3	2	Fe	
24	759·3	92·87	2	2	Fe, Sb	
25	809·5	6277·09	2		An	
26	809·9	76·32				
27	838·2	6215·67	2		Ti, Fe	
28	838·6	14·30				
29	839·2	12·55				
30	849·7	6190·71	2	3	Fe, Sb, C, E.	
31	859·7	69·59	9	1	As, I	
32	860·2	68 48	1		Ca	
33	861·6	65·62	1		N	
34	863·9	62·69	15		Ca, Rb	
35	864·4	61·40				

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No.	Kirchhoff.	Ångström.	Number of times Widnd.	Darknd.	Elements.	Remarks.
36	874·3	40·81	4	4	Ba, Zr	
37	876·5	36·82	13		Fe	
38	877·0	35·82				
39	884·9	21·34	15		Ca, Co	
40	894·9	01·92	15		Ca, Zn, Li	
41	906·1	6077·80	3		Fe, Sb, As	
42	912·1	64·70	3	3	Fe, Ti, Ba	
43	916·3	55·29	2		Fe, Cd, Bi	
44	1002·8	5895·13	12		Na	D_1 once weakened in spot.
45	1005·0	92·10	2		Ni	
46	1006·8	89·12	12		Na	D_2 once weakened in spot.
47	1029·3	56·60		2	Ca, Ni, Mo, Pb, Se, Te	

The Ba line W.L. 6677·6 deserves special notice, as it has been seen several times in the chromosphere during the course of last year, and it is also a marked feature in the spot-spectra.

The chromosphere line K 534·0, which has been occasionally observed in 1883, lies between *a* and B, and is therefore outside that portion of the spot-spectrum which is recorded at this observatory.

In Table II. I have thought it advisable to give the measurements (taken almost entirely from Watts's Index of Spectra) of each element entered in the last column of Table I., in order that the grounds on which their identification rests may be apparent at a glance. In the subjoined list I have included those lines only whose wave-length falls within a tenth-metre of a well-attested measure of some line of an element, or those which are between measures by approved observers, and in estimating the positions of the spot-lines a dispersion of six prisms of 60° has been generally employed, although ten and even twelve have been occasionally tried.

The presence of Ti, Ba, and Ni in sun-spots can scarcely be considered doubtful, although less well-established than that of Fe, Ca, Na, or H. But all the remaining elements decidedly require confirmation; and although some of them rest on very slender foundations, still I have thought it best to adhere strictly to the simple rule of admitting all that satisfy the condition of not differing in their position more than a tenth-metre from the best available measures of the bright lines of the respective elements. From the drawing up of this list it must not be supposed that the evidence adduced would appear to warrant the admission of each named element among solar lines, but it may still be useful to place such close coincidences on record. To avoid needless repetition the authority for each coincident line may be found noted under the special element.

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TABLE II.

Elements observed in the Spectra of Sun-spots between B *and* D.

Element.	Line in spot.	Huggins.	Thalén.	Kirchhoff.	Plücker.	Other Authorities.	Coincident Elements.
Fe	6663·1					A.	
	6593·3					B.	Bi
	92·6						
	74·0					A.	
	43·23					A.	Ti
	6495·12	6497				A. G.	Ba
	94·18		6490	6490			I
	6399·28	6400	6399	6399		A.	
	92·87					A.	
	6212·55					A.	
	6190·70	6190	6190	6190		A.	Sb, C, E
	36·82	6138					
	35·82		6136	6136		A.G.	
	6077·80					A.	Sb, As
	64·70		6065	6065		A.	Ti, Ba
	55·29					A.	
Ca	6717·16	6710		6722		A.	
	6461·98	6458	6462	6462		A.G.	Sb, Cd, Cr
	38·35	6434	6438	6438		A.G.	Cd
	6168·48		6168	6167		A.G.	
	62·69	6163					
	61·40		6161	6161		A.G.	Rb
	21·34	6116	6121	6121		A.G.	Co
	01·92		6102	6101		A.G.	Zn, Li
	5856·60	5854	5857	5857		A.G.	Ni, Mo, Pb, Se, Te
Na	5895·13	5895	5895	5895		A.G.	
	89·12	5889	5889	5889		A.G.	
H	6562·10	6562	6562		6562	A.	
Ba	6677·6	6677					
	6495·12	6499	6496	6497		A.G.	Fe
	6140·81		6141	6141		A.G.	Zr
	6064·70	6064	6062	6063			Fe, Ti
Ti	6543·23		6543				Fe
	6214·30		6214			A.	
	6064·70		6064			A.	Fe, Ba
Ni	5892·10		5892	5891		A.	
	5856·60		5856	5856		B.	Ca, Mo, Pb, Se, Te
N	6482·79	6482	6480	6480			

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Element.	Line in spot.	Huggins.	Thalén.	Kirchhoff.	Plücker.	Other Authorities.	Coincident Elements.
	6165·62				{6165 / 6162} band		
Au	6277·09					B.	
	6276·32	6276	6276	6275			
Sn	6843·8			map			
Bi	6593·3		6599				
	92·6	6590					Fe
	6055·29	6055	6050				Fe, Cd
Sb	6461·98	6461					Ca, Cd, Cr
	6392·87	6392					Fe
	6190·71	6189	6193				Fe, C, E
	6077·80	6076	6078	{6080 / 6076}			Fe, As
Cd	6461·98	6462	6466	{6468 / 6462}			Ca, Sb, Cr
	6438·35	6433	6438	6438		Mascart, 6437	Ca
	6055·29	6050	6056				Fe, Bi
As	6169·59			map			I
	6077·80	6078					Fe, Sb
I	6494·18				6494		Fe
	6169·59				6169		As
C	6190·71					Watts, 6190	Fe, Sb, E
Co	6121·34		6121	6121			Ca
Cr	6461·98	6461					Ca, Sb, Cd
E	6190·71		6190				Fe, Sb, C
Li	6101·92	6096	6102	6101			Ca, Zn
Mo	5856·60		5857				Ca, Ni, Pb, Se, Te
Pb	5856·60	5853	5856				Ca, Ni, Mo, Se, Te
Rb	6161·40			map			Ca
Se	5856·60				5856		Ca, Ni, Mo, Pb, Te
Te	5856·60	5854	5856				Ca, Ni, Mo, Pb, Se
Zn	6101·92	6100	6102	{6106 / 6099}			Ca, Li
Zr	6140·81		6140				Ba

Abbreviations.

A is Angström's Spectre Normal du Soleil.
G is Greenwich Observation of a Sun-spot, 1879.
B is Burton's Catalogue in Brit. Assoc. Report, 1878.

Stonyhurst Observatory:
1884, *Feb.* 21.

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Remarks on Mr. Hilger's Illumination of Micrometers with Vacuum Tubes. By Dr. N. de Konkoly.

I read in No. 82 of the *Observatory* that Mr. Hilger exhibited at the January meeting of the Society a micrometer with vacuum tube illumination.

In the year 1875 I made such a micrometer illumination, but soon discontinued its use, because of the unpleasant shocks which I received from the induction current. I had the eyepiece diaphragm made of ebonite, but then received the shocks in the hand on turning the micrometer. The vacuum tube was entirely enclosed in an ivory and ebonite case on account of the induction current producing such unpleasant shocks. In front of the wires was a very small circular vacuum tube which was kindly made for me by my friend, Professor Lengyel, of the University of Buda-Pest. The wires were bright on a dark ground, and the illumination was very good, without any diffused light in the field, but was unpleasant on account of the tremulous light produced by the interruption of the current. I also illuminated the field of view, so as to produce black wires on a bright ground. My telescope being of metal, we still experienced shocks and also a tremulous light. The shocks might have been avoided by making the eyepiece diaphragm and the button of the micrometer-screw of ebonite, but it was not possible to prevent the tremulous light.

I saw a few weeks ago such a micrometer by Mr. Hilger in the observatory of Cardinal Haywald at Kalocsa, but Dr. Braun, the Director of the Observatory, had also rejected the vacuum tube on account of the tremulous light and the shocks which were received every moment.

I have now illuminated all my micrometers with very small incandescent (Swan) lamps, both on the telescope and spectroscopes. I can obtain from M. Müller, of Hamburg, very small Swan lamps of $\frac{1}{4}$-inch diameter, of 1–2 volts electro-motive power, so that they can be put in action by one or two Bunsen cells or a very small accumulator. The light is very steady, and can be easily regulated by applying different resistances to the current, or by employing diaphragms of oiled paper.

I have made use of this arrangement in my observatory for half a year, and think that no one who has tried the method would ever employ vacuum tubes or oil lamps.

I should be pleased to give photographs or drawings of my improved arrangements to anyone who is interested in the subject.

I may add that M. E. de Gothard has illuminated all the micrometers in his private observatory at Herény on my system.

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Illumination of Spectroscope Micrometers. By Dr. N. de Konkoly.

I take the liberty of calling the attention of the Society to the employment of small Swan incandescent lamps for illuminating the micrometers of spectroscopes.

I have lamps not larger than a hazel-nut, from Messrs. Greiner and Friedrich's, at Stützerbach, Thüringen, and also the size of a very small pea, just enough for illuminating the micrometer of a spectroscope. I have mounted them in a small tube attached in front of the micrometer, so that on the bottom of the tube is a small commutator. As I turn the commutator button 45° I receive light; as I turn it back all is dark. The lamp illuminates also the division of the micrometer-screw.

I have now arranged all my spectroscopes with electric illumination; three of them have a micrometer-screw, and two a scale, except the large Browning, on which both kinds of micrometer are attached. On this latter are two lamps, the one for the scale, the other for illuminating the wires (bright wires on dark ground) and the division of the micrometer-screw.

I send a photograph for the R.A.S., and am sure that Mr. Browning will not know this instrument in its altered form. There is also attached a second telescope parallel to the observing telescope (not attached in the photograph) for reading from the eye-piece the index of the micrometer division.

I have also arranged my large 10-inch refractor for the electric light. I have a large commutator on the eye-end of the telescope, and this communicates with both verniers of the declination circle, reading from the eye-end, with the illumination apparatus of the micrometer wires and the spectroscopes, and also with the R.A. circle.

The electricity is received through the perforated declination axis by an insulated wire from two or four accumulators, made by myself. They are sufficient for one evening's observation, and can be charged during the day by thirty Meidinger elements, the battery of the Observatory telegraph.

Spectroscopic Observation of the Red-coloured Sky at Sunset, 1884, January 9, 5^h 20^m. By Dr. N. de Konkoly.

I used for this observation a Browning pocket-spectroscope, provided with a micrometer, and looked with it at the sky in an altitude between 5° and 10°. I observed in the less refrangible portion of the spectrum (yellow to red) five well-defined bands. The broadest was that towards violet from D, but it was not the darkest. There was a second less refrangible than D, but this was a very indefinite band. At the boundary of the red and orange was a very deep black band, very well defined on both

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sides; less refrangible than that in the red was a very broad but pale band; less refrangible than this, nearly at the limit of the spectrum, was visible a very dark band, the blackest of all.

The positions of the bands were as follows:—

1. from 584·6 to 567·5 *
2. „ 603·1 „ 589·2
3. „ 639·5 „ 627·9 *
4. „ 711·0 „ 661·7
5. „ 739·0 „ 711·9

The correction of the zero point of the micrometer was deduced by observation of the bright sodium line in an alcohol flame.

The bands marked * clearly correspond to the lines of atmospheric vapour.

Spectroscopic Observations of Comet Pons-Brooks made at the Observatory O-Gyalla, Hungary. By Dr. N. de Konkoly.

The comet was observed in different phases of its brightness. I made the first observation on September 27 of the past year, using a spectroscope of little dispersion; I found three very ill-defined bright lines in its spectrum.

The second observation was on November 22 in tolerably steady air, at 5^h 50^m O-Gyalla M.T. The spectrum of the comet was pretty bright, and also the three bands, which were not at all difficult to measure. The position of the bands from the mean of three measures were—

I.	II.	III.
561·0	516·5	471·6 mmm.

I estimated the brightness of the bands respectively as 0·3, 1·0, and 0·5, beginning at the red end of the spectrum, the brightness of band II. being taken as unity.

A faint continuous spectrum was visible between wavelengths 608·0 to 447·4.

I next observed the spectrum of the comet on November 29, 6^h 50^m to 7^h 30^m. Band II. in the spectrum was very bright and remarkable. All three bands were pointed at their ends, and badly defined on both sides, a little swollen in the middle and overflowing in the faint continuous spectrum. This was very defined on band II.

The positions of the three bands from the mean of four observations was—

I.	II.	III.
560·9	516·5	471·4 mmm.

The brightness of the bands was estimated as 0·3, 1·0, and

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0·5. The limits of the continuous spectrum were 616·2 and 434·6 mmm. wave-lengths.

The astronomer of my observatory, M. Rudolf de Kövesligethy, measured band II. on December 7, and found it a little less refrangible than I did (517·3), but M. Kövesligethy supposed it so himself.

On December 23 we measured the positions of the bands in the large 10-inch refractor. We saw a very well defined continuous spectrum corresponding to the nucleus of the comet. The bands were very well defined on the side towards the red and shaded off on the other side. I measured the edge of band II. five times, and the maxima of the light from all the bands also five times. M. Kövesligethy measured also the edge of band II. and the maximum of III. The results were—

	I. Maxim.	II. Edge.	II. Maxim.	III. Maxim.	Observer.
	560·2	519·5	516 8	471·8	Konkoly
	—	519·0	—	473·2	Kövesligethy.
Brightness:	0·3	—	1·0	0·6	—

The observations were made at $6^h\ 40^m$; the comet was 45° above the horizon.

On December 26 M. Kövesligethy observed the spectrum and found the following positions for the bands:—

	I.	II.	III.
	558·5	517·0	477·4
Intensity:	0·2	1·0	0·6

On December 29, $5^h\ 30^m$, I observed the spectrum of the comet with a large Browning spectroscope provided with 2 fine prisms of 60° refractive angle. The spectrum was very bright, and I was able to count six bright bands. Band I*a* was a very faint one, but very well defined at the edge towards the red and shaded off towards the violet. The second band I. (the first ordinary comet-band) was also well defined towards the red, and showed two maxima of brightness. Band II. was very intense, and showed also two maxima, the edge towards the red being the brighter one. The third cometary band was very broad, and had a sufficiently well defined edge towards the red end; there was also a broad maximum. This band still showed two maxima, but they were somewhat faint.

Band IV. had a light maximum on both sides, much shaded off and badly defined; the ends were pointed as the bands of the spectra of very faint comets. Band V. was broad, but very faint; sharply defined towards the red, and shaded off towards the violet. The slit was open 0·4 mm.

The relative intensity of the bands was estimated to be 0·3,

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0·4, 1·0, 0·6, 0·3, 0·1. Clouds prevented me from any series of measures, but I made a good drawing of the spectrum.

M. Kövesligethy made a series of measurements with the other instruments; he found the positions of the three ordinary cometary bands (6-inch refractor).

I. Maxim	II. Edge	II. Maxim	III. Maxim
560·7	518·1	511·6	471·6

On December 30 a set of measures made by M. Kövesgelithy gave—

I. Maxim.	II. Edge.	II. Maxim.	III. Maxim.
558·5	514·8	511·6	478·4

and on December 31 the same observer found—

I. Maxim.	II. Edge.	II. Maxim.	III. Maxim.
558·5	515·2	511·2	472·7

On 1884, January 1, 6^h 10^m, I observed the spectrum of the comet and made a good drawing of it, but clouds prevented me making exact measures. With the universal spectroscope by Merz, with one direct vision spectroscope of 5 prisms on the 10-inch refractor I again saw the six bands just as I saw them on December 29.

M. Kövesligethy made two series of measures, one with the Merz spectroscope on the 10-inch refractor, the other with a smaller spectroscope on the 6-inch refractor. He found the positions—

	I. Maxim.	II. Edge.	II. Maxim.	III. Maxim.
1.	—	—	515·8	470·1
2.	563·0	517·3	511·6	473·3
Intensity:	0·3	—	1·0	0·6

A faint but well-defined continuous spectrum was observed on each occasion.

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Observations of Pons' Comet made at the U.S. Naval Observatory, Washington. By W. T. Sampson and Edgar Frisby.

(*Communicated by the Secretaries.*)

	Wash. M.T.	Comet−✱ Δα	Comet−✱ Δδ	No. of Com.	Comet. α app.	log. (p×Δ)	Comet. δ app.	log. (p×Δ)	Obs.	Com. Star.	Adopted Mean Positions of Comparison Stars. Star.	α 1884·0.	δ 1884·0.	Authority.
1884.	h m s	m s	′ ″		h m s		° ′ ″					h m s	° ′ ″	
Jan. 2	5 46 50·0	+2 31·64	+ 5 45·0	20,4	21 53 30·44	9·503	+20 47 14·0	0·519	S.	1	1	21 50 59·10	+20 41 17·6	Weisse (2) 21, 1209
2	6 0 5·3	−1 24·42	− 0 56·4	20,4	21 53 34·48	9·531	+20 46 20·0	0·531	S.	2	2	21 54 59·18	+20 47 5·0	,, (2) 21, 1318
3	5 39 53·3	−4 11·65	+ 4 37 6	20,4	22 0 35·66	9·475	+19 7 49·4	0·536	S.	3	3	22 4 47·54	+19 3 1·1	,, (2) 22, 67 & 68
3	7 14 8·9	−0 53·65	+ 6 25·3	20,4	22 1 2·87	9·630	+19 1 4·6	0·618	S.	4	4	22 1 56·77	+18 54 28 6	$\frac{\text{Ll.+Rüm.+Lam.}}{3}$
5	5 39 40·5	+1 24·73	− 1 49·6	20,4	22 14 39·15	9·450	+15 38 57·5	0·582	S.	5	5	22 13 14·64	+15 40 37·7	Weisse 22, 233
6	7 8 55·7	−1 18·78	− 0 24·8	20,4	22 21 59·81	9·602	+13 44 2·2	0·653	S.	6	6	22 23 18·77	+13 44 18·4	22, 460
7	7 19 46·1	−2 28·32	− 5 47·1	20,4	22 28 52·50	9·608	+11 52 57·3	0·671	S.	7	7	22 31 20·97	+11 58 36·5	22, 626
9	7 29 7·9	−0 30·58	+ 0 44·9	25,5	22 42 14·26	9·607	+ 8 7 15·7	0·698	F.	8	8	22 42 44·97	+ 8 6 24·4	22, 866
3	20 4·6	−2 38·24	− 3 12·8	30,4	23 7 3·89	9·480	+ 0 37 10·3	0·738	S.	9	9	23 9 42·18	+ 0 40 39·8	23, 145
17	7 27 27·8	+3 30·07	− 1 0·1	15,3	23 30 8·96	9·585	− 6 47 39·9	0·774	F.	10	10	23 26 38·99	− 6 46 40·0	23, 502
20	6 2 16·2	+1 11·30	− 4 42·7	25,5	23 45 20·31	9·419	−11 49 34·8	0·818	S.	11	11	23 44 9·12	−11 44 49·9	23, 868
21	6 0 43·1	−0 58·02	+ 9 32·1	20,4	23 50 10·27	9·415	−13 27 3·4	0·827	S.	12	12	23 51 8·40	−13 36 32·5	23, 1014
21	7 10 53·7	+0 35·75	+16 24·8	25.5	23 50 24·10	9·565	−13 31 23·1	0·806	F.	13	13	23 49 48·47	−13 47 44·9	$\frac{\text{W.+Lamont+Stone}}{3}$
22	7 21 27·7	+0 41·40	+ 1 36·8	20 4	23 55 7·18	9·582	−15 6 33·0	0·807	S.	14	14	23 54 25·91	−15 8 6·2	Arg. Oeltz. S. 23179
25	7 4 26·7	−0 30·76	+ 3 38·6	30,6	0 8 13·98	9·565	−19 30 58·6	0·828	S.	15	15	0 8 44·89	−19 34 31·8	Stone 70
26	6 3 47·0	+1 20·58	+ 1 15·4	20,4	0 12 9·76	9·435	−20 50 6·2	0·858	S.	16	16*	0 10 49·34	−20 51 15·9	Ll. 234
Feb. 1	6 55 21·0	+1 42·42	+ 6 7·0	20,4	0 34 4·88	9·580	−28 9 37·7	0·851	F.	17	17	0 32 22·73	−28 15 36·0	Arg. Oeltz. S. 315

* Bally's reduction of this star is 1′ wrong. Compare also Gould's "Uranometria Argentina."

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Observations of Comet Pons-Brooks, made at Vizagapatam, India. By A. V. Nursing Row.

A comet was in our western sky on the 15th instant, sufficiently conspicuous to the naked eye to attract general attention. I believe it is the same comet that was found in September 1883 by Mr. Brooks in the United States.

At Vizagapatam, sidereal time $3^h\ 58^m\ 4^s$, on the 29th instant, the approximate Right Ascension of the comet was $0^h\ 23^m\ 4^s$, Declination, 24° 5′.

Observatory, Daba Gardens, Vizagapatam:
1884, *Jan.* 31.

Observations of Comet Pons-Brooks made on board the Barque "St. Kilda" on a Voyage from Liverpool to Demerara and Home. By Capt. W. L. Rosseter.

(*Communicated by Capt. Toynbee.*)

Date.	Lat. N.	Long. W.	G.M.T.	Star from which Comet is measured.	Angular Dist.
1884.	° ′	° ′	h m s		° ′ ″
Jan. 14	31 22	54 32	10 29 3 P.M.	Capella	90 13 0
15	31 40	53 44	10 32 20	Aldebaran	79 15 0
17	33 43	49 28	9 59 19	Capella	91 11 0
			10 4 29	Aldebaran	77 44 0
			10 8 0	Polaris	95 23 0
			10 58 23	α Arietis	47 10 0
20	33 53	45 32	10 39 31	Aldebaran	75 45 0
			10 41 47	Capella	92 6 0
			10 44 55	α Arietis	48 0 0
21	36 49	43 31	9 24 0	,,	48 21 0
			9 27 53	Aldebaran	75 15 0
			9 30 46	Capella	92 33 0
22	38 3	41 0	9 8 40	α Arietis	48 48 0
			9 11 3	Aldebaran	74 45 0
			9 13 24	Capella	92 54 0
23	39 5	38 30	9 7 7	α Arietis	49 25 0
			9 9 19	Aldebaran	74 27 0
			9 11 28	Capella	93 20 0
			9 13 11	α Cassiopeæ	72 48 0

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Sextant Observations of Comet Pons-Brooks made at Monte Video. By the Rev. S. S. O. Morris.

(*Communicated by the Astronomer Royal.*)

20th January, 1884.

H.M.S. "Amethyst" at Maldonado at $9^h\ 30^m$ P.M. { Lat. 34° 57′ 0″ S. / Long. 54 57 30 W.

Distance between Comet and *Aldebaran*	75° 29′ 30″	
„ „ *Sirius*	99 10 30	
„ „ *Fomalhaut*	22 14 30	

Resulting position of Comet at $9^h\ 30^m$ P.M. { R.A. 23h 46m 6s / Dec. 12 0 8 S.

21st January, 1884, $8^h\ 56^m$ P.M.

Distance between Comet and *Aldebaran*	75° 1′ 0″	
„ „ *Sirius*	97 36 0	
Position at time of observation	Lat. 34 57 0 S. / Long. 56 3 0 W.	

The New Comet of 1884, *January* 12. By Norman R. Pogson, Government Astronomer at Madras.

A telegram from Kiel Observatory, received on January 16, gave notice of the discovery of a small comet on the 12th, too far south to be observable in Europe, but did not state where or by whom it was found. Circumstances prevented me from looking for the comet until the 21st, when it was speedily recognised. It was round, with a decided condensation towards the centre, and about 90″ in diameter.

Of the following observations, that on January 22 was taken with the eight-inch equatoreal by Troughton & Simms, the six-inch by Lerebours & Secretan being employed for the rest, and a reticle being used in preference to any other form of micrometer available. Moonlight rendered further observations impossible. Corrections for refraction have been duly applied, and all the star positions are from Mr. Stone's Cape Catalogue :—

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	Madras Mean Time.	Apparent Place. R.A.	Apparent Place. P.D.	Parallax Factors. R.A.	Parallax Factors. P.D.	Comparisons.
	h m s	h m s	° ′ ″			
Jan. 22	7 25 10	23 10 20·63	131 57 40″9	+9·8455	−0·5925	5 with 12080
23	7 37 46	23 14 50·88	131 59 13·6	+9·8556	−0·5602	1 each with 12062 12080 and 12105
25	7 31 31	23 23 2 98	131 59 6·9	+9·8504	−0·5779	2 with 12123
26	7 27 28	23 26 50·92	131 58 6·6	+9·8469	−0·5884	1 each with 12105 and 12123
26	7 31 35	23 26 52·47	131 58 31·8	+9·8505	−0·5777	2 each with 12222 and 12226
28	7 14 35	23 33 52·49	131 55 46·7	+9·8355	−0·6178	5 each with 12222 and 12226
30	7 14 24	23 40 20·30	131 51 22·0	+9·8362	−0·6145	10 with 12277
31	7 9 59	23 43 18·29	131 49 15 7	+9·8328	−0·6218	3 with 12277

Madras Observatory:
1884, *Feb.* 7.

Variation in the Light of Neptune, from Nov. 29 to Dec. 14, 1883.
By Maxwell Hall.

Neptune is favourably situated for observation at present, and the telescope was directed to the planet on Nov. 27 in order to detect, if possible, any trace of colour; there were three small stars in the field of view, *Neptune*, and two stars between the eighth and ninth magnitude which we shall call *a* and *b*. Their configuration was noticed in order to identify the planet next night by its motion. On Nov. 28 the stars were observed again, when it was noticed that the brighter and following star had moved so little that its identity with the planet was considered unsatisfactory, and an accurate measure was taken. Repeating the measure on Nov. 29 the identity of the planet was clearly proved; and the delicate bluish colour observed the two previous nights duly confirmed.

This gives us the following remarkable sequence of colour of the planets, proceeding from the Earth outwards:—*Mars*, reddish; *Jupiter*, a delicate orange; *Saturn*, greenish yellow; *Uranus*, light green; and *Neptune*, slightly blue. *Uranus* is seen tinged with green in this telescope; and so indeed I saw it in the magnificent refractor of the United States Naval Observatory. When in Washington early in April 1880, Professor Asaph Hall courteously asked me to the Observatory one night, and showed me *Uranus*: the greenish hue attracted my attention at once, and the satellites *Titania* and *Oberon* appeared to be as conspicuous in that telescope as *Jupiter's* satellites are in my

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own. I would be glad to have this prismatic sequence of colour confirmed, especially with regard to *Neptune*.

Now on the night of Nov. 29 I noticed with surprise that *Neptune* had diminished in brightness and was then no brighter than the two stars *a* and *b*, which had not changed their comparative brightness. These stars have as nearly as possible the same brightness, which has been assumed to be the 8·5 magnitude; and in comparing *Neptune* with the star *a* from time to time, the difference of brightness was *estimated* in parts of a magnitude. The micrometer was applied that night, and assuming that the tabular position of *Neptune* was correct, the common App. R.A. of these two stars was $3^h\ 8^m\ 39^s$ and their App. N. Decl. were 15° 49′ 30″ and 15° 52′ 28″ respectively referred to the Equinox at date. Subsequently a diagonal prism was used to change their relative positions; but by this time *Neptune* was somewhat brighter than *a*, and I began to doubt the correctness of the observations made an hour or so earlier.

The following night was cloudy; and it was not until the night of Dec. 2 that the fact of variation was clearly proved.

Neptune was subsequently observed whenever the weather permitted, and his brightness compared with the star *a* in the same field of view. But stormy weather prevailed, the series of observations was continually broken, and certain photometric appliances could not be used; and so it was not until Dec. 15 that the *period* of variation was determined.

But that night the brightness of *Neptune* remained steady; so again on Dec. 16 and 18; that is to say, the variation had suddenly ceased.

The following table contains all the observations. The first three columns require no explanation; the next column gives in some cases the approximate hour of maximum or minimum deduced from observation by assuming a change from max. 7·5 to min. 8·5 in four hours; and by means of this column all the observations have been combined so as to give epochs of max. and min. The corresponding epochs were then computed by assuming a period of $7^h{\cdot}92$, or

h	m	s
7	55	12,

and by taking Nov. $29^d\ 6^h{\cdot}9$ as the first epoch of min., or Dec. $2^d\ 10^h{\cdot}1$ as the first epoch of max., giving preference to the latter, as Kempshot is $5^h\ 11^m$ west of Greenwich, we have as the max. epoch for future reference:—

d	h	
Dec. 2	15·3	G.M.T.

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Variation of Neptune, Nov. 29 to Dec. 14, 1883.

Local Mean Time.		*Neptune* greater than *a*.	Approx. Mag.	Epochs of Max. and Min. observed.			Epochs computed.		O−C
d	h			h		d h	d	h	h
Nov. 27	9·0	½	8·0						
28	9·0	½	8·0						
29	7·2	0	8·5		Min.	29 7·2	29	6·9	+0·3
Dec. 1	7·8	¼	8·3		Min.	1 6·8	1	6·4	+0·4
2	7·2	½	8·0	9·2					
	8·2	½–¾	7·9	9·8	Max.	2 10·0	2	10·1	−0·1
	9·2	¾	7·7	10·2					
	10·2	¾	7·6	10·7					
	16·2	¼–½	8·1		Min.	2 14·6	2	14·1	+0·5
4	6·0	½	8·0	8·0					
	7·0	½–¾	7·9	8·6	Max.	4 8·9	4	9·7	−0·8
	9·2	¾	7·7	10·2					
6	5·8	¾	7·7	6·8	Max.	6 7·5	6	9·2	−1·7
	8·2	1	7·5	8·2					
7	6·2	½	8·0	8·2					
	8·8	1	7·5	8·8	Max.	7 8·7	7	8·9	−0·2
	10·0	¾	7·7	9·0					
	14·1	¼	8·3		Min.	7 13·1	7	12·9	+0·2
8	7·5	½–¾	7·9		Max.	8 9·1	8	8·7	+0·4
9	6·2	½	8·0	8·2	Max.	9 8·7	9	8·5	+0·2
	9·2	¾–1	7·6	9·2*					
10	7·2	¾	7·7		Max.	10 8·2	10	8·2	0·0
	14·0	½	8·0		Min.	10 12·0	10	12·2	−0·2
12	6·2	½–¾	7·9	7·8	Max.	12 8·0	12	7·7	+0·3
	8·2	¾–1	7·6	8·2†					
14	9·0	½–¾	7·9		Max.	14 7·4	14	7·3	+0·1
	13·5	½–⅜	8·1		Min.	14 11·9	14	11·2	+0·7
15	6·0	⅜	8·1						
	8·5	¼–½	8·1						

The differences between observation and computation given in the last column are small, excepting indeed on Dec. 6, when a difference of $1^h·7$ occurs; but from the difficulties encountered in making the observations, and the varying effects of cloud, twilight, and moonlight, the work taken as a whole is much better than anticipated before the period was determined.

* 8·7 or 9·7; 9·2 adopted.

† 7·7 or 8·7; 8·2 adopted.

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Respecting the cause of this variation, the planet may be assumed to have dark belts on its surface similar to the belts of *Jupiter* and *Saturn*; should these belts break up so as to make one hemisphere darker than the other, the effect of axial rotation would be to produce variations in brightness above and below the mean, as observed; and then the period of variation would simply be the period of axial rotation.

Should such variations be noticed again, we could hardly assume that the positions of maximum and minimum brilliancy on the surface of the planet had remained unchanged; and therefore we could not use the preceding epochs for a more accurate determination of the period, without the greatest care at least.

It may be further remarked that the period is such that if the planet were observed night after night at nearly the same hour, the variations would escape notice under ordinary circumstances; and this allows us to assume that variations have occurred before, and will occur again; so that a watch should be kept upon the planet at hours differing from night to night.

Kempshot Observatory, Jamaica:
1883, *Dec.* 22.

Observations of the Occultation of Venus by the Moon, made at the Royal Observatory, Greenwich, 1884, February 29.

(*Communicated by the Astronomer Royal.*)

	Phenomenon.	Telescope.	Power.	Moon's Limb.	Mean Solar Time of Observation.	Obs.
					h m s	
(*a*)	Disapp. First contact	Simms Eq.	178	Dark	3 2 50 12	W.C.
(*b*)	„	S.E. Eq.	220	„	3 2 51·19	M.
(*c*)	Last contact	Simms Eq.	178	„	3 3 18·12	W.C.
(*d*)	„	S.E. Eq.	220	„	3 3 13·53	M.

Notes.

(*a*) The time for first contact is probably accurate within one or two seconds. A decided piece had been cut off the *s. p.* (bright) limb of *Venus* at 3ˢ after the time noted, when I looked at the chronometer to verify the count of seconds.

(*c*) The time for last contact is, I think, accurate within 1ˢ. The time noted is that at which the last minute point of light had certainly disappeared—it was distinctly seen up to 1ˢ before the time noted, and all was over 1ˢ after, when I looked at the chronometer to verify the count. The Moon's dark limb was not seen, but the bright limb was easily visible, though very much fainter than *Venus*.

Clouds covered the sky till within 2ᵐ or 3ᵐ of the time of first contact, leaving only just time enough to get the planet into the field. W.C.

(*b*) Immediately before first contact, the chronometer was looked at in order to begin a new count, and on looking into the telescope again it was

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immediately noticed that there was a slight flattening of the limb of *Venus*. The dark limb of the Moon came on so fast that it was believed that the contact had been seen from its very commencement, but the time may possibly have been a little late. As the count had only just been commenced, there was no possibility of any error having been made in counting the beats.

(*d*) The last contact was observed at the thirty-first beat after $1^h\ 38^m\ 0^s{\cdot}0$ by the chronometer. The chronometer beats five beats to 2^s; resulting chronometer time is therefore $1^h\ 38^m\ 12^s{\cdot}4$. The chronometer was referred to before $1^h\ 38^m\ 16^s{\cdot}0$ after the disappearance of the last vestige of the planet. Image of planet at moment of occultation, steady; dark limb of Moon sharp and straight. M.

The clear aperture of the object-glass of the S.E. Equatorial is $12\frac{3}{4}$ inches, of the Simms Equatorial 6 inches.

The initials W.C. and M. are those of Mr. Christie and Mr. Maunder.

Royal Observatory, Greenwich:
1884, *March* 14.

Occultations Observed at Forest Lodge, Maresfield.
By Captain William Noble.

Occultation of Venus, 1884, *February* 29.

Clouds were driving across the sky up to the instant of disappearance, which phenomenon was not observed. At reappearance, however, the first glimpse of the planet's limb was caught at $2^h\ 53^m\ 48^s$ local sidereal time $= 4^h\ 18^m\ 15^s{\cdot}0$ local mean time, the gibbous limb of *Venus* separating sharply from that of the Moon at $2^h\ 54^m\ 9^s$ L. S. T. $= 4^h\ 18^m\ 36^s{\cdot}5$ L. M. T. This last determination may be regarded as one of considerable accuracy.

By some curious and inexplicable omission, no mention whatever of this interesting and important phenomenon is to be found in the *Nautical Almanac*.

Occultation of λ *Geminorum*, 1884, *March* 6.

The star disappeared instantaneously at the Moon's dark limb at $9^h\ 10^m\ 15^s{\cdot}4$ L. S. T. $= 10^h\ 10^m\ 5^s{\cdot}7$ L. M. T., and reappeared at the bright limb of the Moon at (very approximately) $10^h\ 12^m\ 30^s{\cdot}1$ L. S. T. $= 11^h\ 12^m\ 10^s{\cdot}2$ L. M. T.

Occultation of κ *Cancri*, 1884, *March* 8.

The star disappeared instantaneously at the Moon's dark limb at $9^h\ 27^m\ 10^s{\cdot}4$ L. S. T. $= 10^h\ 19^m\ 6^s{\cdot}1$ L. M. T. The reappearance was not observed.

In each case a power of 154 was employed on my 4·2-inch

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Ross Equatoreal. From the times given above, 17·8 seconds must be subtracted to obtain the corresponding times at Greenwich, that being the longitude of my Observatory East. Its latitude is 51° 0′ 56″·3 North.

Forest Lodge, Maresfield, Uckfield:
1884, *March* 12.

NOTE.—The Superintendent of the *Nautical Almanac* has informed the President that the calculation of the occultation of *Venus* for Greenwich was made in the usual course. It happened to be upon a form by itself, and was accidentally overlooked in making up the copy of occultations visible at Greenwich for the press. The omission subsequently escaped detection.

Note on the Transit of the IVth Satellite of Jupiter, 1884, *March* 12. By Edmund J. Spitta.

Whilst looking at *Jupiter* on March 12 at 9 P.M. I observed what I took to be a very well-defined shadow. On referring to the *Nautical Almanac* I found it was the IVth satellite itself —pitch black, transiting the disc of its primary, having accomplished about one-third of its course.

I left the telescope, and returning at 11 P.M. again saw the satellite—now more than half way across—as well defined and *black* as before. I note this because I believe it unusual for satellite IV to appear *black*, the colour more often being chocolate or brown. The spot seemed lifted off the planet.

At 11.45, as the time of egress was approaching, the satellite began to lose both its colour and definition; whilst at 11·50 it was still less apparent, being hardly visible five minutes later. I now fancied in the next few seconds I saw the satellite illuminated on the disc, but as the air at the moment was very unsteady I cannot be certain. At 12 o'clock, during a wave of magnificent definition, to my surprise I distinctly re-saw the satellite, almost as black as before, *quite close to the limb*, which I believe in this situation is very unusual. During the next seven minutes it was entirely lost to view, but at 12.7, one minute before the egress, it burst into unmistakable brilliancy.

I did not observe the ingress, but I am informed by one who did, that it was bright until lost in the dazzling splendour of the planet.

Ivy House, Clapham Common:
1884, *March* 13.

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Phenomena of Jupiter's Satellites observed at Stonyhurst in 1883. By the Rev. S. J. Perry, F.R.S.

	Satellite.	Phenomena.	G.M.T. h m s	Corr. of N.A.	Observer.	Remarks.
1883, Jan. 3	II.	Tr. E. Internal contact	5 35 20·9		J.R.	Definition fair. Haze.
		Bisection	37 48·0			
		External contact	40 7·3			
20	I.	Oc. D. External contact	5 40 19·8		J.R.	Definition poor. Haze.
		Last seen	43 45·3			
Feb. 11	I.	Tr. I. External contact	8 22 44·0		W.McK.	Good.
		Bisection	26 38·0			
		Internal contact	29 47·5			
11	I.	Tr. E. Internal contact	10 39 52·9		W.McK.	Difficult. Haze.
		Bisection	43 47·9			
		External contact	47 0·4			
12	I.	Ec. R. First seen	8 59 14·7	− 0m 30s·3	W.McK.	Very satisfactory.
		Half light	9 0 36·1			
		Full light	1 55·9			
16	III.	Oc. R. First seen	7 1 23·6		W.McK.	Good. Haze.
		Bisection	4 3·1			
		External contact	7 4·1			

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	Satellite.	Phenomena.	G.M.T. h m s	Corr. of N.A.	Observer.	Remarks.
Feb. 16	III.	Ec. D. Losing light	9 6 52·6		W.McK.	Excellent.
		Half light	9 24·6			
		Last seen	15 54·6	+ 2 34·6		
16	II.	Oc. D. External contact	11 53 6·1		W.McK.	Haze at beginning. Very good at end.
		Bisection	55 45·1			
		Last seen	59 33·6			
16	III.	Ec. R. First seen	11 53 25·6	− 47·4	W.McK.	" "
		Full light	58 25·1			
18	II.	Tr. I. External contact	6 24 46·5		W.McK.	Very good.
		Bisection	26 46·0			
		Internal contact	28 12·7			
Mar. 4	II.	Tr. I. External contact	11 27 55·0		W.C.	Definition very poor.
		Bisection	29 43·1			
		Internal contact	31 14·5			
5	I.	Oc. D. External contact	11 11 31·2		W.C.	Definition fair.
		Bisection	13 15·4			
		Last seen	14 46·7			
7	I.	Ec. R. First seen	9 15 48·5	− 8·5	S.P.	Very good.
		Half light	17 16·2			
		Full light	18 36·0			

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	Satellite.	Phenomena.	G.M.T. h m s	Corr. of N.A.	Observer.	Remarks.
Mar. 12	I.	Oc. D. Bisection	13 8 38·4		W.C.	Definition very bad. Haze.
		Last seen	9 50·4			
13	III.	Tr. E. External contact	8 54 35·1		J.R.	Definition good. Clouds passing.
14	I.	Oc. D. External contact	7 36 54·0		W.C.	Good.
		Bisection	38 50·5			
		Last seen	40 26·3			
14	I.	Ec. R. First seen	11 11 46·9	+ 0·9	W.C.	Good.
		Half light	12 52·4			
		Full light	17 12·9			
31	II.	Ec. R. First seen	8 35 27·9	+ 13·9	W.C.	Good.
		Half light	37 11·8			
		Full light	38 46·8			
31	III.	Ec. D. Fading	9 12 28·3		W.C.	Very good.
		Half light	15 21·6			
		Last seen	19 1·2	+ 2 20·2		
31	III.	Ec. R. First seen	12 3 33·1	− 1 15·9	W.C.	Good.
		Half light	6 46·7			
		Full light	9 41·7			

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		Satellite.	Phenomena.	G.M.T. h m s	Corr. of N.A.	Observer.	Remarks.
April	4	IV.	Ec. D. Fading	9 29 22·7		S.P.	Very unsatisfactory. The thin cloud over *Jupiter* seemed to absorb the light of IV. more than that of I., II., or III.
			Last seen	53 0·4	+19 6·4		
	4	IV.	Ec. R. First seen	10 45 21·9	−14 26·1	S.P.	Good. Cloudy half a minute later, when IV. still very faint.
	6	I.	Oc. D. External contact	7 56 33·8		W.C.	Fair.
			Bisection	58 3·8			
			Last seen	8 0 9·3			
	6	I.	Ec. R. First seen	11 28 22·8	+ 19·8	W.C.	Definition very bad.
			Half light	29 38·7			
			Full light	30 29·2			
	7	III.	Oc. D. External contact	8 12 49·5		W.C.	Definition very good. Hazy.
			Bisection	17 24·6			
			Last seen	21 29·9			
	12	IV.	Tr. I. External contact	7 23 44·1		S.P.	Good. Twilight strong.
			Bisection	33 42·4			
			Internal contact	47 32·1			
	12	IV.	Tr. E. Internal contact	8 37 54·0		S.P.	Limb fair.
			Bisection	45 40·5			
			External contact	55 0·8			Observation difficult.

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	Satellite.	Phenomena.	G.M.T. h m s	Corr. of N.A.	Observer.	Remarks.
Apr. 15	I.	Ec. R. First seen	7 51 32·4	− 1 0·6	W.C.	Passing clouds. Planet very faint at times.
		Half light	53 7·9			
		Full light	54 44·9			
May 6	III.	Ec. R. First seen	8 10 32·9	− 1 15·1	S.P.	Good. Twilight strong.
		Half light	13 2·4			
		Full light	15 25·2			
16	II.	Oc. D. External contact	9 4 1·9		W.C.	Planet very unsteady.
		Bisection	5 18·4			
		Last seen	11 22·7			
Oct. 5	IV.	Ec. D. Fading	15 41 7·2		W.C.	Excellent.
		Half light	45 14·2			
		Last seen	49 20·6	+ 8 6·6		
5	I.	Ec. D. Fading	16 18 32·1		W.C.	Excellent.
		Half light	19 48·6			
		Last seen	21 1·4	+ 48·4		

Stonyhurst Observatory:
1884, *Jan.* 23.

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Observations of Occultations of Stars by the Moon and of Phenomena Francisco, and at Table Mount Station,

(*Communicated by*

Occultations of Stars

Observed at the *Davidson Observatory*, San

Latitude 37° 47′ 24″·1. Longitude 122° 25′ 37″·6 W.

No.	Date.	Star.	Estim. Mag.	Phase, Im. or Em.	Observer.	Telescope. Aper.	Telescope. Power.	Chronometer.
	1881.					inches.	diams.	
1	June 11	?		Im.	G.D.	6·4	?	Sid. 223
2		c^2 Ophiuchi		,,	,,	6·4	?	,,
	1882.							
3	Mar. 23	ω^2 Tauri	5½	Im.	C.B.H.	3·0	105	Sid. 223
4	28	A′ Cancri	6	,,	,,	6·4	30	,,
5	29	ω Leonis	5½	,,	,,	6·4	30	,,
6	Apr. 19	Jupiter		,,	J.S.L.	6·4	120	,,
7		,,		,,	J.J.G.	3·0	105	Sid. 207
8		,,		,,	,,	,,		,,
9	20	?	8	,,	C.B.H.	3·0	105	Sid. 223
10	28	ϵ Leonis	4½	,,	F.M.	6·4	?	,,
11	May 19	?	7·5–8	,,	C.B.H.	6·4	95	,,
12		?	7·5–8	,,	,,	,,		,,
13		?	9–10	,,	,,	,,		,,
14		?	8½	,,	,,	,,		,,
15		?	8	,,	,,	,,		,,
16		?	7·5–8	,,	,,	,,		,,
17		?	8·5	,,	,,	,,		,,
18		?	8–8·5	,,	,,	,,		,,
19	June 19	B.A.C. 3202?	7	,,	,,	,,		Sid. 207
20	20	19 Sextantis	6½	,,	,,	3·0	105	,,
21	21	55 Leonis	6	,,	F.W.	6·4	30	,,
22		,,	6	,,	C.B.H.	3·0	105	M. 7,4969
23		57 Leonis	7	,,	F.W.	6·4	30	Sid. 207
24		,,	7	,,	C.B.H.	3·0	105	M. 7,4969
25	Oct. 17	?	7½	,,	,,	6·4	95	M. 7,2256
26		?	9–10					
27		?	8					
28		?	7½–8					
29		?	7½–8					
30		?	8–8½					
31		?	8					

There is no explanation of the

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of Jupiter's Satellites, made at Davidson Observatory, San California. By George Davidson.

the Secretaries.)

by the Moon.

Francisco, from 1881, June 11, to 1882, Dec. 31.

(Telegraphically determined). Elevation above the sea 378 feet.

Time as recorded.	Chron. correction.	Local Mean Time.	Local Sid. Time.	Remarks, etc.
h m s	h m s	h m s	h m s	
15 30 1·3	−0 0 16	10 7 8·4	15 29 59·7	During total lunar eclipse.
15 52 28·3	−0 0 1·6	10 29 31·7	15 52 26·7	
7 14 13·9	−0 1 21·8	7 6 52·2	7 12 52·1	Good; instantaneous.
10 16 28·1	−0 1 24·2	9 48 54·6	10 15 3·9	Very good; instantaneous.
9 26 23·0	−0 1 25·2	8 55 0·9	9 24 57·8	Good; disappearance gradual
10 39 31·0	−0 1 50·2	8 44 57·9	10 37 40·8	Entire disappearance.
10 40 22·5	−0 3 8·4	8 44 31·2	10 37 14·1	Contact.
10 40 47·0	−0 3 8·4	8 44 55·7	10 37 38·6	Entire disappearance.
10 57 21 ± 2	−0 1 51·4	8 58 47·8	10 55 29 6	About 11′ N. of centre; very faint and doubtful.
13 27 41·2	−0 1 59·7	10 57 7·8	13 25 41·5	Atmosphere very unsteady.
11 58 45·9	−0 2 25·7	8 5 27·1	11 56 20·2	Very good,* about 11′ N. of centre.
12 4 29·7	„	8 11 10·0	12 2 4·0	„ 11′ S. „
12 6 52	„	8 13 31·8	12 4 26·3 ± 1ˢ·5.	13′ N. „
12 14 30	„	8 21 8·6	12 12 4·3 ± 0ˢ·5.	9′ N. „
12 26 56·5	„	8 33 33·1	12 24 30·8	Good. 6′ N. „
12 37 6 4	„	8 43 41·3	12 34 40·7	Very good. 1′ S. „
12 38 45·0	„	8 45 19·6	12 36 19·3 ± 1ˢ·0.	1′ S. „
12 48 30 7	„	8 55 3·7	12 46 5·0	Good. 4′ N. „
14 31 18·4	−0 7 3·1	8 31 4·7	14 24 15·3	Pretty good.
15 18 41·8	−0 7 6·8	9 14 20·7	15 11 35·0 ± 0ˢ·1	
14 6 30·2	−0 7 10·5	7 58 21·3	13 59 19·7	F.W. satisfied sec. should be 30ˢ·2.
5 13 34·4	+2 44 47·1	7 58 21·5	13 59 19·9	
14 42 2·8	−0 7 10·6	8 33 48·1	14 34 52·2	
5 49 1·1	+2 44 47·2	8 33 48·3	14 34 52·4	
5 28 44·9	+ 36 24·2	6 5 9·1	19 51 2·3	* 8′ N. of centre, approx.
5 33 32	24·2	6 9 56·2	19 55 50 1	Very doubtful 3′ S. „
5 40 26·8	24·2	6 16 51·0	20 2 46·1	6′ N. „
6 26 57·9	24·1	7 3 22·0	20 49 24·7	6′ N. „
6 29 57·8	24·1	7 6 21·9	20 52 25·1	11′ N. „
6 39 45	24·1	7 16 9·1	21 2 13·9 ± 0ˢ·5	15′ N. „
7 2 42·4	24·1	7 39 6·5	21 25 15·1	3′ N. „

Observer's initials in the MS.

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No.	Date.	Star.	Estim. Mag.	Phase. Im. or E.m.	Obser. ver.	Telescope. Aper.	Power.	Chronometer.
	1882.					inches.	diams.	
32	Oct. 17	?	8–8½					
33		15 Sagittarii	5					
34		16 Sagittarii	6					
35		?	8½–9					
36		?	8½					
37	18	?	8	Im.	C.B.H.	6·4	55	M. 7,2256
38		?	8½					
39		?	8½					
40		?	7					
41		?	8½					
42	Nov. 19	9 Piscium	6	Im.	C.B.H.	6·4	95	M. 7,4969
43	Dec. 18	B.A.C. 286	6½	„	„	6·4	?	Sid. 223

Notes.

1 and 2.—Chronometer 223 fast, May 23 = 15ᵐ·6, June 14 = 1ᵐ·63, June 23 = 1ᵐ·72, rate and error derived above from last two observations.

11 to 18.—Chron. error not satisfactory; depends on rate of 223 for a twelve days interval (but from Jan. to July 1882 the daily rate of this chronometer only ranged from 1ᵐ·0 to 1ᵐ·2). On this day, as well as Oct. 17 and 18, the points of disappearance are taken from rough sketch made at the time.

Phenomena of the

Observed at the *Davidson Observatory*, San

	Date.	Satellite.	Phenomenon.	Phase of Phenomenon.	Obser-ver.	Instrument. Aper.	Power
	1882.					inches.	diams.
1 (*a*)	Feb. 21	I.	Ec. reap.	First seen	C.B.H.	6·4	250
(*b*)				Half br.			
(*c*)				Full			
	27	I.	Tr. ingr.	Internal contact	G.D.		250
3	27	I.	Sh. ingr.	„	„		
4	Mar. 5	III.	Tr. egr.	External contact	„		250
	7	I.	Oc. dis.	Entirely covered	C.B.H.		150
6 (*a*)	16	I.	Ec. reap.	First seen	„		?
(*b*)				Half br.			
(*c*)				Full br.			

Remarks.

1. (*a*) Well observed. (*b*) and (*c*) very uncertain. Air somewhat unsteady.
2. Air steady.
3. Air rather unsteady.
4. Atmosphere much disturbed.
5. Atmosphere very much disturbed.

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Time as recorded.	Chron. correction.	Local Mean Time.	Local Sid. Time.		Remarks, etc.
h m s	h m s	h m s	h m s		
7 24 8·5	24·1	8 0 32·6	21 46 44·7 ±	0ˢ·5	8′ N. of centre, approx.
7 46 5·2	24·0	8 22 29·2	22 8 44·9		1′ S. ,,
7 50 1·3	24·0	8 26 25·3	22 12 41·7		13′ N. ,,
7 53 47·8	24·0	8 30 11·8	22 16 28·8		?′
7 55 23·0	24·0	8 31 47·0	22 18 4·2 ±		?′
5 37 25·4	+0 36 22·7	6 13 48·1	20 3 39·2		* ½′ S. of centre, approx.
6 3 31·5	22·6	6 39 54·1	20 29 49·5 ±	0ˢ·2	6′ N. ,,
6 11 29·7	22·6	6 47 52·3	20 37 49·0		5′ N. ,,
6 18 25·9	22·6	6 54 48·5	20 44 46·4		2′ N. ,,
6 30 38·4	22·6	7 7 1·0	20 57 0·9		7′ N. ,,
3 53 45·9	+3 51 59·4	7 45 45·3	23 42 1·3		
1 36 49·4	− 1 47·34	7 44 26·16	1 35 2·6		Well observed.

Notes—continued.

42.—Clock error unsatisfactory. It depends on rate of Sid. clock 1449 from Oct. 28, but clock has been running steadily all year. Rate from June 25 to Oct. 17 = 3ˢ·8 losing daily, Oct. 17 to 18 = 3ˢ·89; Oct. 18 to 27 = 3ˢ·89, and from Oct. 27 to 28 = 3ˢ·93. The star disappeared very close to N. horn and disappearance was to eye of observer gradual: it seemed to "glide in" during about 0ˢ·3. Reappearance not well obtained; first seen at 4ʰ 16ᵐ 36ˢ by chronometer, and estimated time of contact = 4ʰ 16ᵐ·4. On all other dates chronometer error was carefully observed for.

Satellites of Jupiter.

Francisco, during the year 1882.

Chron.	Correction to Chron.	Chron. Time as noted.	Corrd. Local Mean Time.	Time by Amer. Eph.	App. Error of Amer. Eph.
	s	h m s	h m s	h m s	
Sid. 223	−53·5	5 6 4·3	6 57 29·0		
,,	,,	6 30	57 55	6 57 34·4	− 5·4
,,	,,	7 18	58 43		
,,	−59·0	6 46 8·9	8 13 36	8 7 5	
,,	,,	7 59 45	9 27 0	9 23 5	
,,	−66	7 21 48·2	8 25 27	8 23 5	
,,	−67	6 30 50	7 26 45	7 26 5	
,,	−75·8	6 53 35·5	7 13 54·5		
,,	,,	54 45	15 15	7 13 44·3	+10·2
,,	,,	56 0	16 30		

Remarks—continued.

6. (*a*) May be as much as 5ˢ late; sparkle of the satellite seen almost immediately after returning eye to telescope after absence of less than 5ˢ. (*b*) and (*c*) Satellite III. close by for comparison.

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Occultations of Stars

Observed at Table Mountain Triangle, Marin Co., California, by Main Triangulation

Position of Triangulation Station { 37° 55′ 19″·3 / 8ʰ 10ᵐ 23ˢ·5

No.	Date. 1882.	Star.	Estim. Mag.	Phase, Im. or Em.	Observer.	Telescope. Aper. inches.	Telescope. Power. diams.	Chronometer.
1	Aug. 22	B.A.C. 5663?	7	Im.	G.D.	2·0	50?	M. 7,5038
2		,,		,,	C.B.H.	3·0	105	,,
3		?	9	,,	,,	3·0	105	,,
4	24	?	7	,,	,,	3·0	105	,,
5	Sept. 16	B.A.C. 4896, 4900?	7	,,	G.D.	3·0	105	,,
6		,,		,,	J.F.P.	2·0	50	Sid. 223
7		,,		,,	C.B.H.	2·0	50	,,
8	21	?	9	,,	G.D.	3·0	105	M. 7,5038
9		B.A.C. 5920?	7·5	,,	C.B.H.	3 0	105	,,
10	22	B.A.C. 7016?	7	,,	,,	3·0	105	,,

Notes.

1. Star first disappeared behind horizontal cross threads, and immerged while thus hidden; noted distance as above ± 0ˢ·05.

1 and 2. The chronometer error depends on set of 24 equal altitudes of Sun on this day, with rate to Aug. 24, &c. Probable error = ± 0ˢ·1.

4. The chronometer error depends on trial set of * transits by Sub.-Asst. J. F. Pratt.

5. For error of chronometer in this and subsequent observations, see records at Table

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by the Moon.

Party, U.S. Coast and Geodetic Survey, in charge of Prof. George Davidson, Assistant.

Approximate elevation above the sea 2,592 feet.

Time as recorded.	Correction to Chron.	Local Mean Time.	Local Sid. Time.	Remarks, etc.
h m s	m s	h m s	h m s	s
8 15 14·5	+2 28·5	8 17 43·0	18 23 11·1	±0ˢ·5. See note below.
8 15 15·1	+2 28·5	8 17 43·6	18 23 11·7	Pretty good.
9 11 35	+2 28·6	9 14 3·6	19 19 41·0	±1ˢ·0. Very faint and doubtful.
9 5 19·0	+2 34·5	9 7 53·5	19 21 22·9	Dark limb invisible.
6 58 56·2	+3 59·6	7 2 55·8	18 46 45·4	Fog cloud making star and limb of moon very faint, and observation difficult.
18 46 24·5	+0 20·0	7 2 54·9	18 46 44·5	
18 46 24·3	+0 20·0	7 2 54·7	18 46 44·3	
9 14 30·5	+4 9·1	9 18 39·6	21 22 34·3	Very faint; doubtful.
9 45 57·4	+4 9·1	9 50 6·5	21 54 6·4	Good.
8 9 37·3	+4 13·7	8 13 51·0	20 21 31·6	Very good.

Notes—continued.

Mountain Triangle, 1882, time and latitude comparisons, Sept. 16, P.M.—

		h m s	h m s
S.	223 =	18 51 38·0	18 54 38·5
M.	5038 =	7 4 8·0	7 7 8·0

Other comparisons in record mentioned above.

In all except No. 4, the dark border of the moon was visible.

U.S. Coast and Geodetic Survey Office,
Washington: 1884, *Jan.* 22.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLIV. APRIL 9, 1884. No. 6.

EDWIN DUNKIN, F.R.S., President, in the Chair.

Samuel Palmer Chapman, 27 West Park, Clifton, Bristol;
Duncan Forbes, 11 Hanover Buildings, Southampton; and
Charles Horsley, F.G.S., 174 Highbury New Park, N.;

were balloted for and duly elected Fellows of the Society.

Notes on Nyrén's Determination of the Constant of Aberration.

By David Gill, LL.D., F.R.S.

(*Extract from a letter to Professor Otto von Struve.*)

Nyrén's remarks on the possible systematic errors that may attach to determinations of the constant of aberration bring strongly to my recollection the course of thought which led me to write the article which you will find at pp. 40–43, vol. i. of "The Observatory."

I there mentioned that W. Struve's determinations of the constant of aberration exhibited indications of a term depending on the R.A. of the star observed. Nyrén's determinations also exhibit variations which are still more pronounced, and which can be very accurately expressed by terms depending on sin α and cos α—that is by sin $(M+\alpha)$; but the value of M would be quite different if it were deduced from his and from Struve's series.

Struve's series depends on too small a number of stars (notwithstanding the great accuracy of each result) to enable us to arrive at very accurate ideas as to the value of M; still, the fact that M is very different if deduced from Nyrén's series points to the probability that Struve's results (if Nyrén's are accurate) were affected by systematic changes in the clock rate or azimuth, which were more or less eliminated by the methods employed by Nyrén. I quite agree that the precautions adopted by Nyrén of comparing his clock before and after each set of observations with the normal clock (situated in air of uniform pressure, and

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where the diurnal variation of temperature is insensible) enable us to dismiss from further consideration any errors on account of the clock.

But can we entirely dismiss errors depending on the azimuth? If I remember rightly, the marks employed and their lenses were not sheltered from the Sun quite in the same way as the azimuth mark of the Transit instrument; at least we have no detailed account of their construction.

Also, since azimuth marks in the prime vertical and the ground on which they are situated are exposed to greater causes of disturbance in azimuth than similar marks situated in the meridian, it is of the utmost importance that all care should be taken to obtain evidence of their freedom from diurnal variation.

[To explain further what I mean, the Pulkowa meridian marks and the soil on which they stand are exposed chiefly to the Sun from the south, which would tend to create their chief variations in the direction N. and S. (I speak of the diurnal effect as a whole), which, therefore, might tend to change their azimuth more in a N. and S. direction than E. and W.]

Now, as you have two and not four marks, you cannot from the marks alone separate the change of azimuth from change of collimation. But it would be of great interest to compare *separately* the azimuths of the two marks, employing the value of the collimation derived from the star observations, which collimation might be assumed to have a constant relation to the value of the collimation when the instrument is horizontal (of course you could not assume the collimation identical for the vertical and horizontal positions of the telescope). If by this means it could be shown that the relative azimuths of the marks show no diurnal variation of mutual azimuth, the determination as a whole, it seems to me, would have even a higher value —high as the present value, in my eyes, is. Of course I admit that the effect of any such systematic error is nearly (but not quite) eliminated by the distribution of the stars in R.A.; and I agree with Nyrén in the opinion that the definitive result of his researches is, on this account, more accurately estimated by giving equal weight to the results derived from stars in each quadrant of R.A., than by giving each separate result a weight corresponding to its probable error.

But if M has a real existence, it is a fact of the highest cosmical interest, and I think that no trouble should be spared to determine it with the highest possible accuracy.

If M were accurately determined at Pulkowa, and another value M^1 as accurately determined here, we should have from the two mean values of aberration and the values of M and M^1 the necessary data for determining whether or not the galaxy has a motion of translation in space, at least within certain limits; and I see no reason to suppose it impossible that that translation may amount to thirty or forty times that of the Sun's

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translation relative to neighbouring stars—at least, I think Nyrén has dismissed the matter somewhat too absolutely. There is at least in his results very strong evidence of the existence of M, and this should be traced to its source.

With this in view I venture to suggest the following points beyond a further examination of the question of azimuth.

1. What are the parallaxes of the stars employed?

2. What are the colours of the stars?

1. The determination of the parallaxes of the stars employed in the research, relative to those of stars of 7th or 8th magnitude with a good heliometer, could be accomplished in two years with all desirable accuracy. I would write further on this point but that I have said all that I can say on this subject in the memoir which I mention in the accompanying letter, and which I hope to send you in a few months after I reach England.

2. If there be anything in Forbes's researches (which I am inclined to doubt on the evidence of Newcomb*), red and blue light have sensibly different velocities, and it would be very interesting to see what evidence Nyrén's investigations give on this point. If the redder stars give a smaller value for the constant of aberration than the whiter or bluer stars, it would be a fact of much importance.

Royal Observatory, Cape of Good Hope:
1883 *Dec.* 19.

Remarques sur les "Notes on Nyrén's Determination of the Constant of Aberration" de M. Gill. Par M. Nyrén.

Il m'a été de beaucoup d'intérêt de lire les remarques faites par M. Gill sur mon mémoire, "L'aberration des étoiles fixes." Quant aux doutes qu'il exprime sur quelques points du mémoire, je m'empresse de répondre ici en quelques mots.

Par rapport aux mires destinées à contrôler la constance en azimut de notre instrument du premier vertical, il a été dit dans mon mémoire que leur construction a été tout à fait analogue à celle des mires de la lunette méridienne. Les piliers portant les mires sont construits en briques et s'enfoncent de 10 pieds dans le sol, étant isolés du terrain environnant par un intervalle assez large. Les parties supérieures des piliers sont entourées deplanches, et l'espace entre les planches et les piliers est rempli d'étoupe de chanvre. Des maisonnettes en bois revêtues de mortier entourent l'ensemble de ces constructions. Autour de ces cabanes il y a des arbres en partie très vieux. Pour la mire d'Est, les arbres n'abritent pas complètement la maisonnette

* Newcomb said when here, that, with his powerful apparatus, if there had been a difference of $\frac{1}{1000}$ part in the velocity of red and blue light, his star (or spot of light) whose displacement, due the time of passage of light, he observed, would have been converted into a spectrum that could not have escaped his attention.

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contre les rayons du Soleil; à l'Ouest, ce n'est qu'exceptionnellement qu'un rayon pénètre jusqu'à la maisonnette et le sol voisin. Les piliers dans la salle d'observation, portant les objectifs collimateurs, sont fondés de la même manière que ceux des instruments; ils sont toujours abrités contre l'insolation.

Avec ces précautions je le regardais comme assez sûr qu'aucune variation appréciable de la ligne de collimation de nos mires ne pouvait s'effectuer dans l'intervalle d'une ou de deux heures, entre les passages de la même étoile par les deux branches du premier vertical. Pour cette raison j'ai pensé que les détails des lectures des mires pourraient sans inconvénient être réservés à la publication des observations *in extenso*, procédé qui me parut d'autant plus justifié qu'avec les matériaux à notre disposition aucune sûreté absolue sur la variabilité dans les piliers des mires ne peut être gagnée. Cependant, les remarques de M. Gill me font croire qu'il serait peut-être opportun de donner un peu plus de détails sur cette question: je m'empresse donc de communiquer quelques résultats ultérieurs de mes études sur les lignes de collimation des mires et de l'instrument. Puisqu'il ne s'agit que des variations périodiques de ces lignes, je ne m'occuperai pas ici de leurs valeurs absolues.

Désignons, dans la position de l'instrument au Sud du pilier, par S_e et S_o les moments des passages d'une étoile par un fil du réticule dans les deux branches Est et Ouest du premier vertical; par N_e et N_o les moments des passages, observés le même jour, par le même fil, dans la position Nord. En supposant que l'erreur de collimation du tube et l'inclinaison de l'axe horizontal soient restées constantes entre les passages en Est et en Ouest, on devrait trouver dans nos observations:—

$$\frac{S_e + S_o}{2} - \frac{N_e + N_o}{2} = 0.$$

En admettant que, dans l'intervalle entre les passages par les deux branches du premier vertical, l'erreur de collimation ait changé de la quantité dc, on aura approximativement:—

$$dc = \sin \phi \cos \delta \sin t \,.\, dt,$$

où

ϕ = latitude

δ = déclinaison de l'étoile,

t = angle horaire pour le passage par le pr. vert.

$$dt = \frac{S_e + S_o}{2} - \frac{N_e + N_o}{2}$$

Une valeur positive de dt indiquera que l'angle formé par l'axe horizontal et l'axe optique du côté de l'oculaire du tube a augmenté.

Les dc déduits par cette voie de toutes nos observations ont été comparés avec les variations dm, fournies directement par les lectures des mires faites immédiatement avant et après les

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observations. Pour les différents quadrants en ascens. droite on a trouvé ainsi en moyenne :—

		Matin.	Soir.
h h		"	"
0–6	$dc-dm$	$= -0.24$	$+0.04$
6–12	,,	-0.28	-0.05
12–18	,,	-0.06	-0.05
18–24	,,	-0.15	-0.19

Si l'on voulait attribuer ces différences à des variations de la ligne de collimation des mires, ce seraient les différences Matin-Soir dont on devrait tenir compte dans la déduction de l'aberration. On aurait donc :—

		Matin−Soir.		Décl. moy. des étoiles observées.
h h		"	"	° ′
0–6	$dc-dm$	$= -0.28$	± 0.058	59 10
6–12		-0.23	0.065	59 13
12–18	,,	-0.01	0.064	59 9
18–24	,,	$+0.04$	0.060	59 1
Moyenne ...	...	-0.120	± 0.031	59 8

Les observations étant réduites dans la supposition que les mires soient restées immobiles entre les passages Est et Ouest, il serait donc possible que les observations faites aux environs du maximum (ou minimum) de l'aberration exigeraient encore une correction moyenne de $0''.006$, correction de beaucoup inférieure à l'err. pr. $\pm 0''.015$, trouvée pour le résultat de notre dernière série d'observations. Mais tant que nous ignorons dans quelle mire la variation ait eu lieu, ou, ce qui revient au même, lorsqu'on ne veut admettre que l'instrument soit resté invariable en azimut, nous ne pouvons même dire si cette correction devait être positive ou négative.

Lorsqu'on compare, comme le propose M. Gill, les quantités dc avec les dm déduits de chaque mire séparément, on trouve comme variations horaires pour les différents mois :—

		Est.		Ouest.	
		Matin.	Soir.	Matin.	Soir.
		"	"	"	"
Janvier	$dc-dm$	$= -0.07$	$+0.13$	-0.15	-0.13
Février	,,	0.00	$+0.02$	0.00	-0.02
Mars	,,	$+0.11$	$+0.18$	-0.19	-0.14
Avril	,,	-0.11	-0.01	-0.15	-0.35
Mai	,,	$+0.15$	$+0.12$	-0.01	-0.52
Juin	,,	-0.38	$+0.15$	-0.56	-0.39
Juillet	,,	$+0.01$	-0.08	-0.29	-0.06
Août	,,	-0.02	$+0.10$	-0.26	-0.16
Septembre	,,	$+0.05$	-0.04	-0.23	-0.14
Octobre	,,	$+0.02$	$+0.25$	-0.16	-0.31
Novembre	,,	-0.21	-0.27	-0.53	-0.31
Décembre	,,	—	-0.37	—	-0.01

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En apparence les valeurs plus fortes de ces différences pour la mire Ouest devraient conduire à la conclusion que cette mire, plus que l'autre, ait été sujette à des variations de position, ce qui paraît peu probable, vu qu'elle est si bien abritée contre les rayons du Soleil. Mais au lieu d'attribuer les écarts à une cause si peu probable, et dont l'existence n'est pas démontrée, il paraît plus raisonnable de se tenir à des explications basées sur des phénomènes déjà bien constatés. Comme tels phénomènes il faut mentionner ici :—

1°. Variations de l'instrument en azimut, distinctement prononcées dans les lectures des mires.

2°. Différences systématiques entre les *dc* déduits de la manière expliquée précédemment, et les variations réelles de l'erreur de collimation de l'instrument. La vérité de cet énoncé est démontrée par le fait que les valeurs moyennes des *dc*, déduites des observations commencées avec l'instrument Sud du pilier, ne s'accordent pas avec les *dc* fournis par les observations commencées Nord. En combinant les *dc*, pour chaque quadrant, en deux groupes selon la position primitive de l'instrument, on trouve :—

	Matin.			Soir.		
	dc_s	dc_N	$dc_s - dc_N$	dc_s	dc_N	$dc_s - dc_N$
h h	"	"	"	"	"	"
0–6	−0·49	−0·10	−0·39	+0·07	+0·11	−0·04
6–12	−0·52	−0·05	−0·47	+0·13	+0·21	−0·08
12–18	−0·18	−0·06	−0·12	+0·18	+0·40	−0·22
18–24	−0·36	+0·04	−0·40	−0·19	+0·10	−0·29

Puisqu'il est inadmissible de supposer que les variations réelles de l'erreur de collimation soient différentes pour les deux moitiés du réticule, les écarts $dc_s - dc_N$ portent à la conclusion que ni l'une ni l'autre valeur de *dc* ne coïncide avec la variation réelle de la dite erreur, et, par conséquent, que rien ne s'oppose à la supposition que cette variation soit en réalité exprimée par les valeurs trouvées de *dm*.

L'explication des différences notées doit être cherchée, sans doute, dans l'équation personnelle de l'observateur concernant les passages par les fils du réticule. Pour rendre cette explication plus probable, il suffit de mentionner que la dite équation peut varier par plusieurs causes : par la vitesse du mouvement de l'étoile, par la direction de ce mouvement, par la position de l'observateur, tous changements qui arrivent dans chacune de ces observations, mais qui se combinent de différentes manières si l'observation est commencée au Sud ou au Nord du pilier. Les différences des valeurs $dc_s - dc_N$ pour les différents quadrants pourraient bien être accidentelles.

M. O. Struve a trouvé pour les variations de la collimation produites par des changements de la température, la valeur +0″·087 pour −1° R. (Obs. de Poulk., vol. iii. p. 201), et presque la même

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valeur résulte de notre série d'observations si on la déduit de la même manière. La prépondérance du signe positif pour les valeurs de dc, déduites des observations du soir, est donc en bon accord avec cet effet de la température. Mais aussi pour les observations du matin on devait attendre le même signe, vu que la température s'est abaissée en 200 cas environ, et n'a monté qu'en 50. Mais comme on voit, l'effet des autres causes mentionnées a prévalu pour cette époque du jour. On retrouve un phénomène analogue dans la série de W. Struve. En déduisant les dc, pour les deux époques du jour, des observations isolées—non par la comparaison d'observations différentes, instituées en différentes saisons—on trouve pour dc_m (matin)—dc_s (soir), et les changements correspondants de la température v_m (matin)—v_s (soir), les valeurs moyennes :—

	$dc_m - dc_s$	$v_m - v_s$
	″	°
β Cassiop.	−5·95	+2·71 R.
δ Cassiop.	−1·72	+1·57
ι Dracon.	−1·61	+0·07
b Dracon.	−1·95	+1·33
o Dracon.	−3·15	+0·98
Ceph. 2 H.	−1·35	+2·21

On voit donc qu'aussi dans cette série les variations $v_m - v$ ne suffisent pas pour expliquer les différences $dc_m - dc_s$.

Il se présente maintenant la question très importante si les variations supposées de l'équation personnelle ont pu exercer une influence sensible sur les distances zénithales observées. S'il en était ainsi, on pourrait attendre que les observations commencées au Sud du pilier donneraient en moyenne d'autres résultats que dans le cas où l'on a commencé au Nord. C'est pour diminuer l'incertitude qui en résulterait que, presque sans exception, on a suivi la règle de commencer les observations alternativement au Sud et au Nord. En combinant, sous ce point de vue, toutes les différences en déclinaison : observation—calcul, en deux groupes, on trouve l'écart au commencement Sud—commencement Nord :—

		Matin.	Soir.
h h		″	″
0–6	S–N	= +0·034	+0·055
6–12	„	+0·011	+0·002
12–18	„	+0·003	+0·021
18–24	„	+0·033	+0·077

Il s'ensuit qu'aussi sans cette précaution, le résultat pour l'aberration aurait été presque identiquement le même. Ce fait nous permet de conclure que les variations supposées dans l'équation personnelle s'éliminent à peu près complètement dans les observations isolées.

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Concernant la remarque de M. Gill sur le point de mon mémoire, où j'exprime des doutes sérieux sur la possibilité de parvenir à la connaissance du mouvement absolu de translation du système galactique par des déterminations de la constante de l'aberration en différents endroits du ciel, c'est une question qu'on ne peut, *à priori*, ni démontrer ni nier absolument. Je me bornerai donc à dire, qu'entre les différentes explications qu'on pourrait proposer pour rendre compte des différences entre les valeurs de l'aberration trouvées dans notre série, par des étoiles de différentes ascensions droites, ce ne serait qu'avec la plus grande précaution qu'on puisse accepter celle qui voudrait attribuer les dites différences au mouvement du système galactique. Il me paraît plus probable que les saisons y ont joué un rôle important.

Note to Accompany Correspondence on the Subject of Nyrén's Determination of the Constant of Aberration. By David Gill, LL.D., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope.

Nyrén's recent determination of the constant of aberration with the Prime Vertical Transit Instrument at Pulkowa, and his discussion of the values of the same constant as determined with the Transit Instrument and Vertical Circle of that Observatory, together constitute one of the most interesting and valuable contributions to modern astronomy.

The wonderful agreement of the results obtained by the three instruments in question, from different stars and totally different modes of observation, affords the most striking testimony not only to the genius of the elder Struve, who planned the instruments and methods of work, but also to the zeal and ability of the astronomers who have so efficiently carried on and refined the traditions of high accuracy that have always been associated with the work of Pulkowa. In writing on these and other matters to Otto von Struve I was anxious to obtain further information as to certain points in M. Nyrén's work, because it seemed not improbable that germs of results even still more interesting might be found in it. I accordingly enclosed in my letter the above notes for M. Nyrén's perusal, but without any intention of publishing them in their present form. In reply to these notes M. Nyrén has made some further investigations, the results of which he has kindly communicated to me, and they are so interesting that (with M. Nyrén's consent) I forward them for publication in the *Monthly Notices*.

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Observations of Comet a 1884, *made at Windsor, New South Wales.* By John Tebbutt.

On the evening of January 14 I received from the Melbourne Observatory a telegram to the effect that a small comet was found there on the 11th. Its position and daily motion for the 12th were communicated, but owing to clouds and smoke I did not succeed in finding the comet till the 19th. It was then just beyond the reach of unassisted vision. In the telescope it presented itself as a round nebula, about two minutes of arc in diameter, with a bright condensation in the centre. There was no definite nucleus in the micrometer eye-piece, but a short tail could be distinguished. The comet faded very rapidly, and was observed with extreme difficulty on February 2. The accompanying places have been deduced from comparisons made with a square bar micrometer on the 4½-inch equatorial. In bringing up the mean places of the comparison stars from the various authorities the precessions and secular variations have been adopted from Stone's Cape Catalogue. With the exception of star No. 6 no reliable authority exists for proper motion. The proper motion of this star is very large, and has been taken from that Catalogue. I trust at a future opportunity to be able to send a determination of the orbit. Considering the rapid fading of the comet from our view it is probable that the heliocentric motion is retrograde. I commenced observations of Pons' comet on January 13.

Apparent Places of Comet 1884.

Windsor Mean Time. 1884.					R.A.			Log. $\frac{p}{r}$	N.P.D.			Log. $\frac{q}{r}$	No. of Comps.	Comp. Star.
	d	h	m	s	h	m	s	+	°	′	″	+		
Jan.	19	9	37	42	22	54	35·76	8·8669	131	46	56·3	9·6972	6	1
	19	9	37	42	22	54	35·55	8·8669	131	46	56·5	9·6972	6	2
	21	9	26	7	23	5	2·58	8·8712	131	54	59·2	9·6657	4	3
	21	9	31	39	23	5	3·91	8·8701	131	55	2·7	9·6781	5	4
	22	9	3	4	23	9	44·99	8·8735	131	57	21·3	9·6073	8	3
	22	9	3	4	23	9	44·93	8 8735	131	57	19·5	9·6073	8	4
	23	9	22	11	23	14	18·89	8·8726	131	58	45·3	9·6531	5	3
	23	9	22	11	23	14	18·79	8 8726	131	58	43·8	9·6531	5	4
	24	8	51	49	23	18	29·18	8 8729	131	59	15·1	9·5742	5	3
	24	8	51	49	23	18	29·05	8·8729	131	59	12·6	9·5742	5	4
	25	9	14	35	23	22	34·15	8·8734	131	59	19·6	9·6338	1	3
	25	9	14	35	23	22	34·13	8·8734	131	59	18·7	9·6338	1	4
	27	8	50	59	23	29	57·41	8·8727	131	57	33·4	9·5731	2	3
	27	8	50	59	23	29	57·32	8 8727	131	57	31·6	9·5731	2	4
	28	8	56	48	23	33	26·24	8·8731	131	55	54·1	9·5907	4	5
	28	8	56	48	23	33	26·07	8·8731	131	55	50·9	9·5907	4	6
Feb.	2	9	38	9	23	48	48·21	8·8661	131	44	19·1	9·7002	3	7

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Mean Places of the Comparison Stars for 1884·0, with the Reductions to the Apparent Places for the Dates of Observation.

Star.	Mean R.A.	Reduction.	Mean N.P.D.	Reduction.	Authority for Star's Mean Place.
	h m s	s	° ′ ″	″	
1	22 49 17·36	−0·93	131 42 57·1	+5·4	Cordoba Zone 29, No. 51; Cape Cat. 1880, No. 11941.
2	22 53 50·84	−0·92	131 46 18·8	+5·7	Wash. Cat. 1860, 2nd ed., No. 10109; Cordoba Zone 29, No. 53; Cape Cat. 1880, No. 11966.
3	23 8 32·22	−0·88	131 44 0·2	+6·5	Wash. Cat. 1860, 2nd ed., No. 10232; Melb. Cat. 1870, No. 1183; Cordoba Zone 29, No. 66; Cape Cat. 1880, No. 12062.
4	23 10 25·93	−0·87	131 49 39·8	+6·6	Wash. Cat. 1860, 2nd ed., No. 10249; Cordoba Zone 29, No. 67; Cape Cat. 1880, No. 12080.
3	23 8 32·22	−0·89	131 44 0·2	+6·4	See authorities above.
4	23 10 25·93	−0·88	131 49 39·8	+6·5	„ „
3	23 8 32·22	−0·90	131 44 0·2	+6·2	„ „
4	23 10 25·93	−0·89	131 49 39·8	+6·4	„ „
3	23 8 32·22	−0·91	131 44 0·2	+6·1	„ „
4	23 10 25·93	−0·90	131 49 39·8	+6·2	„ „
3	23 8 32·22	−0·92	131 44 0·2	+6·0	„ „
4	23 10 25·93	−0·92	131 49 39·8	+6·1	„ „
3	23 8 32·22	−0·94	131 44 0·2	+5·7	„ „
4	23 10 25·93	−0·94	131 49 39·8	+5·8	„ „
5	23 31 50·93	−0·88	132 12 28·1	+7·3	Cordoba Zone 51, No. 25; Cape Cat. 1880, No. 12222.
6	23 40 23·59	−0·84	132 11 53·8	+7·9	Melb. Obs. 1874, No. 404; Melb. Obs. 1875, No. 341; Cordoba Zone 51, No. 37; Cape Cat. 1880, No. 12277.
7	23 45 39·68	−0·85	131 28 14·3	+7·5	Cordoba Zone 51, No. 46; Cape Cat. 1880, No. 12319.

Windsor, N. S. Wales,
February 12, 1884.

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Observations of Comet Ross made at the Observatory, Melbourne. By R. L. J. Ellery, F.R.S.

Date, Melb. M.T. 1884.	Δα	Δδ	App. α.	App. δ. South.	Comparison Stars. α	Comparison Stars. δ	Authority.
d h m s	m s	′ ″	h m s	° ′ ″	h m s	° ′ ″	
Jan. 12 9 3 29	+4 31·0	+ 0 5·6	22 3 37·0	40 7 14	21 59 6·0	40 6 8·4	Cape Cat. 11603
17 9 16 31	+4 28·3	+ 7 19·6	22 42 35 2	41 30 57	22 38 6·9	41 22 37·4	Cape Cat. 11850
18 8 31 25	+0 38·0	− 2 42·0	22 48 38·5	41 40 22	22 49 16·5	41 43 3·9	Cape Cat. 11941
28 10 41 30	+1 0·0	−11 15·1	23 33 45·0	41 55 54	23 32 45 0	42 7 9·1	Washington 10436
29 9 29 3	−3 29·6	−17 46·4	23 36 53·0	41 54 10	23 40 22·6	42 11 56·4	Cape Cat. 12277
Feb. 1 9 25 2	+0 18·3	+15 27·7	23 45 57·1	41 43 47	23 45 38·6	41 28 19·3	Cape Cat. 12319
4 9 24 0	−0 24·5	+18 12·3	23 54 9·0	41 38 26	23 54 33·7	41 20 13·7	Cape Cat. 12395
7 8 44 31	−2 16·2	− 2 32·0	—	—	0 3 38	41 33 47	Anonymous

Cloudy weather came on after the 7th, and there was no really clear sky on any evening until the 19th, when a very rough measure was obtained viz. :—

R.A. ... h m s 0 25 50 Decl. ... ° ′ ″ 40 56 0 S.

The comet was then so faint as to be seen with difficulty, and no glimpse of it has been obtained since, owing to continual cloudy evenings. It was rapidly increasing its geocentric distance from January 12.

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both pair of openings by means of little milled heads, *g g g* (figs. 2 and 3) behind the micrometer box, so that if required either or both systems of wires can be eclipsed or both illuminated. A further advantage is afforded by this part of the apparatus, which is often an advantage in observing faint objects: the declination or micrometer wires proper can be *half eclipsed*, so that the position of an object may be got by pointing the end of the illuminated wire (which is mid-field) to the object. Objects that are invisible behind a faintly illuminated wire can be thus plainly seen. Of course, by shutting off the lamp L, and taking illumination from the ordinary central mirror of the telescope, a bright field with dark wires results.

In micrometer work it is often very inconvenient to be obliged to hold a lamp to read off the micrometer head, and to obviate this the following arrangement is made. In the screen at H (fig. 2) is a central aperture which allows a beam to pass into the tube t^2 (fig. 2), carrying a small mirror *m*, which reflects the light into another small mirror (not seen in section), thence to a dead white surface at *n* (fig. 2), giving a nice illumination of the divided heads, which can be read comfortably and conveniently with the little lens *o* (fig. 2). This lens folds back, to be out of the way of the face when using high powers.

There is, however, one inconvenience in this form of micrometer: the distance from the plane of the webs to the plane of the eye-piece slide makes it necessary to bring the eye very close to the plate for the highest powers, but this inconvenience is only relative and presents no absolute difficulty.

As regards the electrical illumination of the telescope, there are five incandescent lamps used in divided circuit, one for the micrometer, as described, one for the declination setting circle, one for microscopes of declination circle, one for central illumination and for position circle, and one for illumination of hour circle microscopes and reader. All these are connected with a small commutator and rheostat, fixed to the declination sector, with a small ebonite handle easily reached while observing. With this arrangement, by turning the handle, any one of the lamps is put in circuit, and the amount of light regulated by pulling or pushing a light-sliding tube on the commutator handle.

The current for the lamps is obtained from a battery of four carbon-zinc elements in bichromate fluid and cells, arranged in cascade. As this form of battery is found very successful for the purpose, it may be useful to describe the arrangement.

Four carbon-zinc elements are mounted on a frame, each set easily removable. This frame is suspended and counterpoised over four large glass cells, arranged in steps, so that when the fluid is 6′ high in the first cell it overflows into the second, and so on, and from the fourth it overflows into a portable vessel. The source of fluid supply is a lead-lined box of about four gallons capacity, to which is fitted a glass stop-cock for regu-

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lating the flow. The square surface immersed of each element is about six square inches, and the delivery of fluid from two to four quarts per hour, when a very constant and steady light is given. When not in use the elements are lifted out of the cells and the flow of the fluid stopped. The fluid requires renewing as soon as all warmth of colour by transmitted light disappears, but it will produce a steady light until every bit of bichromate is decomposed; by simply increasing the flow as the fluid becomes weaker it will do a great many hours' work before it requires renewing.

Suggestions for Improvements in the Construction of Large Transit Circles. By A. A. Common.

The ordinary well-known form of Transit instrument, considered in regard to the purpose for which it is used, may be taken as the best designed astronomical instrument we have, fulfilling as it does, when of moderate dimensions, nearly all the required conditions. Its chief defect, inherent in the construction usually adopted, is that of flexure of the tube, and in a less degree of the axis itself. If this flexure of the tube is equal on both sides the result is only a slight movement of the optical axis parallel to itself, but if it is unequal then a change of direction necessarily follows. In either case the accuracy of the observation is not affected.

Most probably such permitted flexure would result in some change of direction of the optical axis in a plane not at right angles to the axis; but, at the worst, this resulting error is all that the Transit instrument suffers from.

When, however, this instrument, good as it may be for its own purpose, is made into a Transit Circle by placing on the axis a divided circle for the purpose of determining the direction of the optical axis in a vertical plane, without in any way altering the construction to get rid of the defect spoken of, that would here become of great importance, it cannot be said that all has been done to make it as perfect or as suitable for its purpose as possible, especially as the delicacy and refinement of observation require that the means of the astronomer should be as mechanically perfect as they can be made, so as to reduce to the lowest amount those errors that must always exist in an astronomical instrument.

Believing, as I do, that the Transit Circle, as at present made, is capable of great improvement, I wish to lay before the Society some remarks on the construction usually followed, and to offer some suggestions for consideration.

If we examine the construction of this instrument we find that, generally speaking, it is only an enlargement of the

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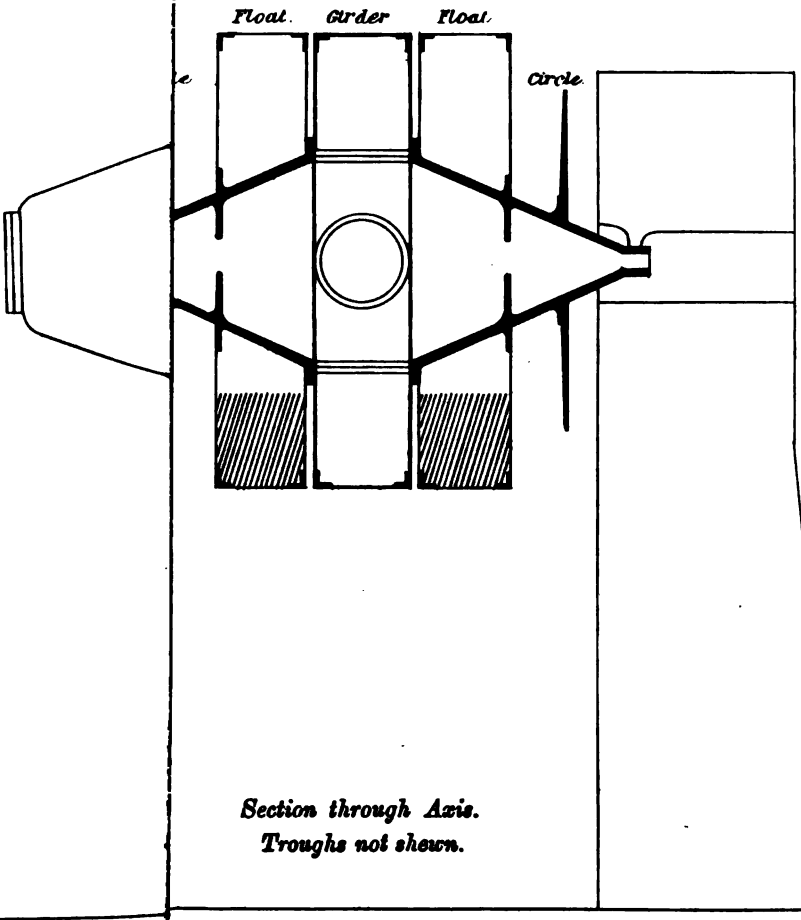
ten feet focal length.



Section through Axis.
Troughs not shewn.

Spottiswoode & Co. Lith. London.

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ordinary Transit instrument, with the addition of a divided circle and means to read it, and that there is not any real change of plan or evidence of any attempt to meet the altered conditions of the case. Rigidity is expected from increase of metal; but, as I shall show presently, this increase is, in most cases, not so well disposed as in the model. The usual construction is something like this. There is a hollow metal cube, from two sides of which spring the cones, terminating in the pivots; this forms what may be termed the axis; to the other two opposite sides of the cube are attached, by flanges, the tubes carrying the optical points of the instrument. The remaining sides of the cube are perforated for the use of the collimators. The connection of the tubes to the cube by flanges and bolts is general, and raises the question as to the effect of bolts if screwed up unequally.

Now, if we take a section of such an axis, we find that the metal does not, at the angles formed by the cube, cover the lines of compression or tension, as the case may be, but is outside these lines—to my mind as fatal a disposition as can be imagined for such a case. If we place such an instrument with the tube horizontal what do we find takes place as the result of gravity? The hole in the upper face of the cube becomes elliptical with the major axis in the direction of the tube, for the tension of the upper sides of the tube tend to pull it so, assisted by the compression of the upper parts of the cones at right angles to this direction, the reverse action going on below; and, further, the elbows formed by the corners of the cube and the junction of the flanges with the tubes alter their angle under the stress, and the consequence is flexure, principally in the plane at right angles to the length of axis, but not either equal or confined to this plane, and necessarily of a very uncertain character, and in large heavy instruments of considerable amount. The flexure that would, in a small instrument, be lost sight of, not only from its small amount, but from the want of power in such an instrument to detect it, becomes, in a large instrument, greater in proportion to the size, and more evident in proportion to the increased power.

The connection of the divided circle (and here I would like to say that in my opinion the making of circles like wheels, that is with spokes and a rim, is wrong altogether) by which the zenith distance of the optical axis is determined is not of such a nature as affords the best results, nor can it be where the flexure of the tube is admitted, and the rigidity of the circle is presumed. Properly, there should not be any more possibility of change in one more than in the other.

The counterpoise friction wheels are usually applied near the pivots, and not, as they certainly should be, as near as possible to the centre of gravity of each half of the axis; and are all of a mechanical character, that is, wheels pressed up by weights and springs.

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The pivots invariably work in Ys, either with flat faces or concave for a certain amount to fit the pivots, and oil is used between the surfaces.

In making an observation, as far as I know, the determination in declination is made by bringing the fiducial wire as near the star as possible, and determining the exact position of this fiducial wire, representing the optical axis of the telescope, by reading many microscopes, and then measuring the distance of the star from this fiducial wire by the eyepiece microscope.

There are other matters, more of detail but not less important, well worth remark, but as they are not affected by the alterations I go to propose, they need not now be touched upon.

The symmetrical form of the Transit instrument has so many advantages over any modification of the mural circle form (and there are many that suggest themselves on thinking over this subject), particularly in the power of reversal, that it should certainly be retained. Keeping as near as may be to it, I would suggest the following alterations from the plan already mentioned. (Without going too minutely into details that could only be properly dealt with by working drawings, I have endeavoured to show these at a glance in the accompanying sketches, giving farther on some figures to show size and weight.)

The main axis should be, as in the Transit instrument, of two cones, but these I would connect base to base by an overlapping ring-joint. This joint would be cut in four places to allow for the light from telescope and collimators to pass. To this axis, in place of the usual tube, I would attach two thin discs of metal, about one-tenth less in diameter than the focal length of object-glass, and at a distance apart a little more than its aperture. In the case of the focal length exceeding a moderate amount—say six or seven feet—I would use, in place of the discs, sheets shaped somewhat in the manner shown in the sketches.

These discs or sheets are to be connected by an outside band and radial plates, and to be securely fastened to the axis already named by flanges in a vertical plane.

These discs, bands, and radial plates form practically a box-girder, the depth of which in the smaller size is equal to its length, and in the larger sizes is some fraction not less than one-half.

For the purpose of supporting these discs or sheets and the axis, in place of the usual mechanical counterpoise, I would have two short hollow iron cylinders immersed in their lower part in troughs of mercury to such an amount as to give the requisite lifting power, which in this construction would be slightly greater than the whole moving mass. These cylinders would be rigidly connected to the axis and the discs or sheets, and they would be placed so as to be as near as possible to the centre of gravity of each half, to reduce all strain on the central parts as much as possible. These floating counterpoises, while not so neat-looking as the mechanical ones,

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have so many advantages in their freedom from vibration, their delicacy and constancy, and above all their perfection as reducers of friction, that they are quite worth the sacrifice to appearance their use demands.

The troughs for the floats to work in would be supported from below by suitable means to allow for the lowering and raising, running out, reversal, and return of the instrument.

The divided circles may be, in the case of a moderate-sized instrument, the discs themselves, or they may be, as is usual and more convenient, fastened to the axis near the pivots. In any case the circles should have a lenticular section, or be like a plate dished out a little, and not any kind of wheel with spokes and rim.

The pivots and their supports, determining as they do the absolute position of the instrument, are of the first importance. In place of the usual Ys, I would suggest that plane surfaces be used, one horizontal and one vertical, for each pivot bearing; these planes, of some hard stone, such as black carbonate, embedded in metal blocks, and brought to a true plane surface, the pivots being kept up to contact with the horizontal plane, and not, as is usual, brought down upon it; the contact of the pivot with the vertical plane being by any suitable means, as, for instance, a slight spring or a small pressure-wheel, exerting sufficient force to overcome any tendency of the pivot to roll instead of slide on the plane when the instrument is turned. In such a case the whole of the base parts supporting the troughs would have to be counterpoised by gravity, so as to have the requisite pressure to ensure contact between pivot and plane. It will be seen that in this arrangement the duty of the pivots is only to fix the plane of rotation of the instrument, and that they can never be subject to the whole weight of the moving part, but only to the amount of pressure spoken of, that is, sufficient to ensure contact; they are in this way relieved from the possibility of undue strain, the whole weight being always borne by the floats even in the reversal. The finishing of the pivots would be done after the instrument is permanently set up.

In the ordinary form the pivots have not only to do this duty spoken of, but they have to support more or less of the whole weight, assisted by the counterpoises, but always with the probability that at some time this whole weight may be brought on them, while in lifting or reversal, the pressure is generally applied at some fresh part of axis, so bringing new strains to work. It is doubtful if such an instrument would not be permanently deformed if once left to support itself on the pivots.

The fixing of the object-glass in its cell, and the preservation of the optical axis, that is, the line joining the optical centre of object-glass and fiducial lines in eye end, from change due to alteration of temperature, would seem to be best met by engraving these lines on a piece of glass similar in size to the

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object-glass, and held, as regards its power of expansion or contraction, or the effect of this on the holder, in the same way as the object-glass. Such lines ruled on glass would be in every respect better than spider web, as not having any flexure, whilst being much finer. The use of graphite instead of oil as a lubricant would seem to have advantages in all cases, but more particularly where there is some little surface of contact, as when the Ys are made concave to suit the pivots, and would be well worth trial in existing instruments.

The use of an ordinary level would be impossible owing to the great size of the central parts between the pivots, but much might be done to get over this difficulty by using a column of mercury under each pivot, connecting the two columns below; in fact, using a U-shaped tube of a suitable width, filled with mercury, and provided with some means of causing the mercury to rise at will so as to show convex above the tops of the tubes. On placing this level so that each tube comes within about one-tenth of an inch of the underside of pivot, and applying pressure, the rise of the mercury could be watched, and the level of the two pivots got fairly well. If such a level had at the lower part of the U an enlargement in which could be placed a vertical septum of some flexible but not elastic material, that would serve as an insulator to keep the two columns electrically separate without affecting them otherwise, the contact of each column could be determined in a very exact way by a very weak battery and a fine galvanometer placed in circuit with the pivot and column of mercury, the contacts taking place at each pivot and showing on the galvanometers at the same instant if the pivots are level, when the mercury is slowly forced up. Such a level could be used with the ordinary form of Y, and in either case could be a permanent fitting. The pivots would have to be free from oil, of course, to ensure electrical contact, but the use of graphite would be favourable to this plan.

Taking the sketches given to roughly represent the moving parts of a Transit Circle of ten inches aperture, and ten feet focal length, with an axis six feet long, with a box-girder in place of discs to carry the optical parts, and provided with two circles of solid steel about three feet six inches in diameter, and allowing one hundred pounds for object-glass and eye-end fittings, we have the following approximate weights of the moving part, all in steel:—

	lbs.
The axis of cast steel 30″ diameter in two lengths with flanges	1100
The central part of steel one-fifth inch thick, including all angles, partitions, and fittings	1050
Two cylinders 56″ diameter, one-eighth metal, including all angles, etc.	700
Two circles, 42 diameter	350
Clamping gear	150
	3350

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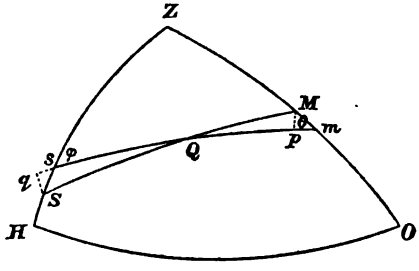
This is not an excessive weight for such a large instrument, and it is rather a question whether it might not be increased considerably. The displacement of about five cubic feet of mercury would equal it, and the cylinders of a length of one foot would give this amount of displacement with a margin. There are no mechanical difficulties in such a construction, though there are points that would require careful consideration. It allows of all the corrections and adjustments that the present instrument allows, and in the case of a small instrument where discs are used in place of what I have termed the box-girder; if required, more than one telescope, that is O. G. and eye-piece, could be used; and even a double telescope, at right angles to the principal telescope, that is, one where the O. G. of one is in front of the eye-piece of the other, and the lines of reference can be engraved on the object-glass of each; or one at some required angle from the first, for differential observations. The friction being so reduced the setting of the instrument actually on the star to be observed could be easily effected with proper clamping and moving arrangements.

A Method for Clearing a Lunar Distance. By John Merrifield.

Let Z be the zenith, HO the horizon, m and s the apparent places of Moon and other object, M and S their true places;



then $m\,s$ is apparent distance, M S the true. With pole Q describe small circles S q and M p, then

$$qp = \text{MS, the true distance.}$$

Now if

d' be the apparent distance,
d „ true „
C „ correction for ☽'s altitude
c „ „ ☉ „

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then

$$\begin{aligned} SM &= qp = sm - pm + sq \\ &= d' - C \cdot \cos\theta + c \cdot \cos\phi \\ &= d' - C\left(1 - 2\sin^2\frac{\theta}{2}\right) + c\left(1 - 2\sin^2\frac{\phi}{2}\right) \end{aligned}$$

$$\therefore\ d = d' - (C - c) + 2\left[C \cdot \sin^2\frac{\theta}{2} - c \cdot \sin^2\frac{\phi}{2}\right] \ldots 1.$$

Let

m = moon's apparent altitude. z = moon's apparent zenith distance.

s = other objects' „ „ z' = other objects' „ „ „

$$S' = \frac{z + z' + d'}{2} \quad \text{and} \quad S = \frac{m + s + d'}{2}$$

Now

$$\begin{aligned} \sin^2\frac{\theta}{2} &= \frac{\sin(S' - d') \cdot \sin(S' - z)}{\sin d' \cdot \sin z} \\ &= \frac{\sin\frac{z' + z - d'}{2} \cdot \sin\frac{z' + d' - z}{2}}{\sin d' \cdot \cos m} \\ &= \frac{\sin\left[90 - \frac{m + s + d'}{2}\right] \cdot \sin\frac{m + d' - s}{2}}{\sin d' \cdot \cos m} \\ &= \cos S \cdot \sin(S - s) \cdot \operatorname{cosec} d' \cdot \sec m. \end{aligned}$$

Similarly

$$\sin^2\frac{\phi}{2} = \cos S \cdot \sin(S - m) \operatorname{cosec} d' \cdot \sec s.$$

From 1

$$\begin{aligned} d &= d' - (C - c) + 2\cos S \cdot \operatorname{cosec} d' \left[C \cdot \sin(S - s) \cdot \sec m - c \cdot \sin(S - m) \sec s\right] \\ &= d' - (C - c) + 2\cos S \cdot \operatorname{cosec} d' \left[M - N\right] \end{aligned}$$

if

$$M = C \cdot \sin(S - s) \sec m,$$

and

$$N = c \cdot \sin(S - m) \sec s.$$

Of course a similar formula could be obtained by taking

$$\cos\theta = 2\cos^2\frac{\theta}{2} - 1 \text{ and } \cos\phi = 2\cos^2\frac{\phi}{2} - 1 \text{ in 3rd line.}$$

EXAMPLE.—The apparent altitude of the Moon is 54° 17′ 46″, that of the Sun 63° 18′ 48″; the apparent central distance is 41° 17′ 14″. The Sun's correction for altitude is 25″, the Moon's correction 33′ 24″. Find true distance.

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$m = 54^\circ\ 17'\ 46''$ sec ·233888

$s = 63\ 18\ 48$ ... ... sec ·347646

$d' = 41\ 17\ 14$ ... ... cosec ·180565

2) 158 53 48

$S = 79\ 26\ 54$ ... ... cos 9·262741

$S - s = 16\ 8\ 6$ sin 9·443891

$S - m = 25\ 9\ 8$ ... ... sin 9·628414

$C = 33'\ 24'' = 2004''$ log 3·301898

$c = 25$ ... ... log 1·397940

$M = 954\ 28$ log 2·979677

$N = 23{\cdot}66$ log 1·374000

$M - N = 930{\cdot}62$ log 2·968772

$C \cdot \sin\frac{\theta}{2} - c \cdot \sin\frac{\phi}{2} = 258{\cdot}3''$ log 2·412078

2

$516{\cdot}6 = +8'\ 36''{\cdot}6$

$C - c = -32\ 59$

$-24\ 22{\cdot}4$

$d' = 41\ 17\ 14$

True distance $d = 40\ 52\ 51{\cdot}6$

Borda's method gives ... ... $d = 40\ 52\ 58$

Krafft's „ „ ... ... $d = 40\ 52\ 30$

This question is taken at random from a work on Nautical Astronomy.

Abnormal Appearance of Jupiter's Satellite IV. while in Transit on March 12. By Henry Pratt.

At 10.30 P.M. the satellite was seen as a conspicuous spot on the great white belt just south of the equatorial zone, and it was observed until 11^h with power of 270, maintaining the whole time the same appearance, which I think is unusual even among the many dark transits which have been observed. Its form was the most singular feature, for instead of being round it was much elongated (elliptical or gibbous), and its major axis was not quite perpendicular to *Jupiter's* equator, but slightly in-

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clined to it. It may be well to mention that this distortion of form was not caused by maladjustment of my speculum, because I have an original apparatus for testing that, and for adjusting by when needed, and which reports most sensitively the slightest deviation. I knew, therefore, the cause was not instrumental.

The general tone of the satellite was very dark, but not nearly as black as a shadow, although in the absence of such a comparison it might have been supposed to be black by an inexperienced observer. Its preceding margin and northern end were marked by a really black shading, which had very much the form of an inverted comma. Had this black shading been of the nature of a dark limb, it should have appeared on the opposite side of the satellite.

18 *Preston Street, Brighton.*

Occultations of λ *Geminorum and* κ *Cancri.* By Henry Pratt.

Occultation of λ *Geminorum on March* 6.

It occurred about the predicted time, but I omitted to note time precisely, owing to an unexpected phenomenon. The disappearance was observed by a power of 270 focussed very sharply on the star. Definition fair, but at times slightly unsteady.

While preparing to note the time of disappearance a diminution of the star's brightness suddenly commenced, and automatically at same instant I commenced counting beats of the sidereal clock. When I had counted the third second the star quite suddenly disappeared completely, and during the interval, which I estimated $= 3^s{\cdot}2$, its brightness was *gradually diminishing*, until when last seen it was not more than one-fourth of the brightness it exhibited before the phenomenon began. I watched for half a minute, but no reappearance or projection on Moon's limb took place. The reappearance occurred within the opened webs of the micrometer, but was not seen until the star had emerged about 1^s, owing to much rippling of the Moon's limb which commenced a few minutes previously.

Occultation of κ *Cancri on March* 8.

The disappearance was observed through haze with power 270 focussed (as sharply as the state of definition would allow) upon the star. It occurred at $10^h\ 18^m\ 46^s$ G.M.T., and was what is

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usually termed perfectly instantaneous. Certainly it did not occupy ·2 of a second, but I thought it took place by a gradual diminution of brightness, although the time was so very short. Reappearance unseen, because at that time clouds covered the Moon.

18 *Preston Street, Brighton.*

The Radiant Points of Fireballs. By W. F. Denning.

There are certain dates of the year when fireballs have displayed a marked ascendency, and when their radiant points have become determinable from the path directions of such as have been accurately recorded. The catalogues of various meteor observers comprise a large number of these observations, though the relative proportion of meteors equalling or exceeding *Jupiter* is somewhat less than three per cent. of the aggregate number registered. In addition to this source of information there are the Annual Reports of the British Association Committee on Luminous Meteors, in which are published a large number of important computations (chiefly by Prof. A. S. Herschel) in connection with the heights, velocities, and radiant points of fireballs. Combining these results with my own and other observations of meteors during recent years, I have endeavoured to find the positions of some of the leading fireball radiants of the year.

In an investigation of this kind many difficulties have to be confronted, for a considerable number of instances are met with where the evidence of certain showers cannot yet be regarded as proved with sufficient distinctness. It is also doubtful, owing in many cases to insufficient materials, as to which are the systems entitled to precedence. There is also another point of signal importance with respect to the *durations* of these displays of large meteors. At first I attempted to group the radiants into very short periods of about two or three nights, but found the observations incompatible with such limited duration. In some well-defined examples they appear to extend over several weeks, and in fact present an analogy to showers of ordinary shooting stars, with which they are no doubt intimately associated; and as further records accumulate, the probable identity of such streams will be yet more closely established. As evidence of the long duration referred to, I may mention the shower in Scorpio (June–July) and Gemini (Nov.–Dec.), which obviously continue in action during a lengthy interval, as the following instances of doubly-observed fireballs (computed by Profs. Niessl and Herschel respectively) will sufficiently prove:—

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Date.			Radiant. α	Radiant. δ	Date.			Radiant. α	Radiant. δ
			h.	°				h.	°
1883	June 3	...	249·9	−20·2	1862	Nov. 27	...	102	+26
1883	3	...	248	−20	1877	Dec. 9	...	112	+27
1878	7	...	249	−21	1863	12	...	105	+27
1873	17	...	248·6	−20·2	1873	24	...	115	+38
1879	July 13	...	246	−19					

The latter is also very rich in October, at the epoch of the *Orionids*, when it furnishes some very swift and, occasionally very brilliant, meteors in the morning sky :—

Radiant Points of Fireballs.

No.	Epoch.	Radiant. α	Radiant. δ	Approximate Star.
		h.	°	
I.	Jan. 1–3	230	+51	β Boötis
II.	4–12	57	−12	γ Eridani
III.	Mar. 1–4	300	+80	κ Cephei
IV.	30–Apr. 7	27	+18	α Arietis
V.	Apr. 11–12	106	+46	16 Lyncis
VI.	19–20	274	+37	α Lyræ
VII.	19–23	258	+37	τ Herculis
VIII.	19–23	210	− 9	κ Virginis
IX.	29–May 5	335	− 9	θ Aquarii
X.	June 3–7, 17	247	−25	α Scorpii
XI.	July 25–30	326	− 9	β Aquarii
XII.	25–30	8	+34	β Andromedæ
XIII.	25–31	338	−11	δ Aquarii
XIV.	25–31	303	+12	α Aquilæ
XV.	25–Aug. 1	32	+53	χ Persei
XVI.	Aug. 9–11	4	+56	η Persei
XVII.	9–11	12	+30	β Andromedæ
XVIII.	9–11	335	+73	β Cephei
XIX.	9–11	284	+62	o Draconis
XX.	9–11	312	+50	α Cygni
XXI.	19–22	330	+69	β Cephei
XXII.	19–25	291	+60	δ Draconis
XXIII.	Sept. 1–4	306	+54	α Cygni
XXIV.	6–15	61	+36	ε Persei
XXV.	10–25	336	+35	π Pegasi
XXVI.	Oct. 2	225	+52	β Boötis
XXVII.	15–22	92	+15	ξ Orionis

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No.	Epoch.	Radiant. α	δ	Approximate Star.
XXVIII.	15–22	106°	+23°	ε Geminorum
XXIX.	17–22	40	+20	ε Arietis
XXX.	17 26	247	+41	η Herculis
XXXI.	25–Nov. 7	48	+22	ε Arietis
XXXII.	Nov. 5–8	58	+16	γ Tauri
XXXIII.	12–14	148	+23	γ Leonis
XXXIV.	19–23	62	+22	ε Tauri
XXXV.	27	29	+46	γ Andromedæ
XXXVI.	Dec. 6–8	80	+23	ζ Tauri
XXXVII.	6–12	106	+32	α Geminorum.

Several of the above showers ought perhaps to have been coupled together as identical. XV. is certainly an early display of the Perseids (XVI.). The Andromedes (XII. and XVII.), Cepheids (XVIII. and XXI.), Draconids (XIX. and XXII.), Cygnids (XX. and XXIII.), Muscids (XXIX. and XXXI.), and Taurids (XXXII. and XXXIV.) are so nearly conformable to the same respective positions that it is impossible to resist the inference that these pairs are closely allied, if not actually connected, with each other.

I have solely relied on my own observations and reductions for the positions given in the table. In cases where the same radiant has been determined by other observers the agreements are usually very satisfactory, though not, in two or three instances, as close as might be expected. Thus No. IX. is Col. Tupman's shower of May 2, 1870, which he placed at 325°−2½°, so that there is a difference of about 11° between the two results. The June shower in *Scorpio*, which seems a very important one, is given by Prof. Niessl at 248°−20,* but I judged it to lie at 247°−25° from the large fireball which I, in common with many others, observed on June 7, 1878, $9^h\ 50^m$. The July Aquariads (Tupman, No. 43), at 341°−13°, which returned with great activity in 1878 and few following years, are doubtless identical with the fireball radiant No. XIII., which has been derived from a variety of sources. I have not included in the list such radiants as have been ascertained by duplicate observations of isolated fireballs, of which there are probably some hundreds. It will be found, however, that many of the showers now given receive close confirmation from such observations. As to the showers in *Perseus* (No. XXIV.), in *Musca* (No. XXXI.), and near ζ *Tauri* (No. XXXVI.) they seem to have escaped special

* *Astronomische Nachrichten*, No. 2566. Prof. Niessl's computation of the position of this radiant is probably very exact as the several observations of large fireballs which he has discussed show an excellent corroboration.

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notice until the last few years, although they severally furnish displays of considerable richness.

In addition to the centres included in the summary, there are other showers which unquestionably deserve a place, though it seems necessary not to deviate from the original plan of giving none but those of the most prominent character. One or two instances may, however, be adduced as representatives of a numerous class:—

January 12–19, from *Cancer*.—The large fireball of Jan. 12, 1879, had a radiant at 133°+19°. Another fireball, seen on Jan. 19, 1877, was directed from 135°+27°, and probably emanated from the same system.

February–March.—Some large fireballs from *Sextans* (145° +6°).

March.—Fine meteors diverge from S. of *β Leonis* (175° +14°).

May 30.—A fireball radiant in *Cassiopeia* (18°+58°).

June–July.—Several fireballs from the northern limits of *Sagittarius* (285°−12°).

Such references to showers more or less certainly established from past observations might be greatly extended through each of the last six months of the year, until on Dec. 30 we come to a display from the extreme north-east region of *Auriga* (92°+57°), to which the great meteor of Dec. 30, 1878, owed its origin.

There is also a notable fireball epoch on Feb. 7–10 and Dec. 21, but I have failed to discover the places of radiation from a sufficient number of observations.

I had intended to annotate the positions included in the table, with remarks explanatory of their probable strength and the sources of corroboration, but the addition of such matter must have extended the paper to considerable length beyond its purpose. It will be noticed that all the well-known periodical showers are represented in the list. I found it necessary to include them, as they have each, at their several epochs, supplied many fine meteors, though neither the Orionids or Andromedes are remarkable for their fireballs. The special nights of recurrence of all the chief periodical showers are, however, those upon which very extended watches have been kept, and when the rich showers alluded to have asserted their supremacy over the more attenuated contemporary displays not only as regards shooting stars of normal aspect, but (from their much greater intensity) as regards large meteors also.

At some epochs prolific in fireballs, such as those of March 1–4 and April 11–12, it has been found difficult to decide as to the principal radiant point, and the positions adopted in these cases must be understood as provisional, and subject to such alteration as future observations may render necessary. Early in March there are several large meteor showers proceeding from *Leo*, *Ursa*, and *Camelopardus*; and about April 11–12 the observations clearly indicate the positions of three radiants,

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in addition to that given in the list, at 70°+50°, 159°−8°, and 18°+57°. In these and other similar cases the want of additional data is greatly felt; not only is it required in support of showers tolerably well known, but of others whose existence our scanty records have given us only the mere suspicion. Amongst the latter there are probably many important systems, and hence the desirableness of further pursuing these investigations. The present catalogue is preliminary to what it is hoped future observations will enable to be greatly extended and revised. The necessity for closer attention to this interesting branch of astronomy is admitted, and its great significance has long since been recognised.

I believe these fireball radiants are equally numerous with the star showers of ordinary character, and are often identical as regards physical origin. With reference to the Taurids of November I have observed several instances of minute telescopic meteors and of the largest type of fireballs belonging to this shower. The epoch and place of the radiant agree so closely that, notwithstanding the enormous difference of magnitude in the phenomena observed, there can be no question that they are derived from the same original source. With respect to the number of the positions comprised in the list, they might have been greatly augmented by the addition of many streams which have manifested themselves with more or less certainty in recent years. But it seemed desirable to omit these, awaiting further confirmation, and thus avoiding, as far as possible, the risk of pseudo-radiants, or of radiants of such markedly feeble character as rarely to give any visible indication of their presence.

In observations of fireballs there is a great need of uniform and accurate description. Amongst observers so widely different as regards astronomical knowledge, and so variously impressed as to the essential features of such records, we cannot wonder that their results are often very discordant, and sometimes of no practical utility whatever. The observer is generally taken unawares, and seldom manages to obtain good positions of a fireball's track (even if he be practised in such work), unless by means of the enduring streak, which so frequently remains as an unfailing indication of it. But this can only be considered as such in the first few seconds of its projection, before it will have commenced to drift and assume that contorted appearance which is so common a feature of these meteoric afterglows. I have sometimes noticed a considerable drift in fireball streaks, and the grotesque forms they occasionally put on before ultimate extinction are very striking. Observers should, therefore, note the path from the direction of the streak in its primitive stage, and prolong the extremities (in estimating the whole length of the arc traversed by the nucleus) a sufficient extent on marking the course upon a chart or globe. The greatest intensity of the streak invariably occurs at the point of maximum outburst, near

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the end of the visible flight of the meteor, and the streak is often divided into sections or much condensed in certain parts, according to the remarkable fluctuations which frequently occur in the brightness of the nucleus as its derivative source. The point of greatest brilliancy of a meteor will, therefore, be found to conform with the brightest region of the streak. It is always best to give the path in R. A. and Decl. rather than attempt description by certain neighbouring stars, the latter method being less accurate and more troublesome in reference. Nor should the meteor be described by altitudes or compass bearings, unless in cases where correctness is ensured by instrumental comparisons, or where the apparition occurs in daylight or hazy weather, when it is not feasible to otherwise assign places. Ordinary estimations of altitude are enormously in excess, and it is often doubtful, with regard to given compass-bearings, whether they are true or magnetic. Whenever the observer is not sufficiently versed in the constellations to assign the path in any other manner than by random guesses at the altitude and azimuth, it is best to reject such observations, unless by combination with many other results, which may serve as a corrective. By comparing several observations, in which the alt-azimuthal points of a fireball have been carefully measured, they will doubtless exhibit an excellent consistency, enabling the radiant point and other elements to be ascertained with precision; but in general it must be admitted that a uniform method of recording meteor flights by degrees of R. A. and Decl. + or −, is preferable to any other, as avoiding the confusion and troublesome references involved in different modes of description.

The systematic watching for very large meteors, when not undertaken in conjunction with that of ordinary shooting stars, is likely to exhaust the patience of the most enthusiastic observer, and is therefore not to be recommended. Meteoric observations are best followed on the approved plan, which admits the necessity of noting the faintest objects visible. And these observations should be sustained irrespective of special epochs and the paths and durations of meteors registered in a convenient form. What would accelerate the development of our knowledge of fireballs more than perhaps any other circumstance would be the *general* attention of astronomical and meteorological observers, could we command it, to the accurate description of the more salient characteristics of these phenomena. Were such a plan followed a few years would obtain for us an enormous increase in our records, and enable many doubtful points to be cleared up. Observers would do well to practise themselves in this department of research, and to contribute more freely to its slowly accumulating facts. Co-operation in a sphere of this character where the individual objects to be observed are incapable of definite prediction either as regards time or place of appearances is very essential, for even in the

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habitual observation of fireballs a single observer will gather comparatively few materials unless his labours are prolonged over a very long series of years. The work to be thoroughly successful must be undertaken at many different stations. This has, indeed, to some small extent, been already effected, and as the result we have the valuable meteor catalogues of Heis, Schmidt, Zezioli, Weiss, Konkoly, Schiaparelli, Denza, and others. But notwithstanding this diligence at a few isolated observatories, our existing materials are, it must be confessed, deficient in the extreme, and quite inadequate to the purpose of a satisfactory discussion. Such systems of fireballs as we have already been enabled to detect by means of the few scattered data in our possession may be fairly assumed to constitute streams of great importance, and to afford some notable examples of the multitude of such showers which are displayed with more or less distinctness every year.

Bristol, 1884 *March* 15.

Some Remarks on the Chain of Meridian Distances, measured around the Earth by H.M.S. "Beagle" between the years 1831 *and* 1836. By Prof. A. Auwers.

A great difference between some recent determinations of longitudes in the Straits of Magellan and the results, apparently well founded, and regarded as fundamental up to the present day, in the geography and hydrography of South America, obtained by H.M.S. "Beagle" half a century ago, has been the reason of my undertaking some fresh computations concerning the chronometrical measurements of differences of longitudes which formed one of the main objects of the celebrated second cruise of the said vessel, under the command of Capt. R. Fitz-Roy, during the years 1831 to 1836.

Fitz-Roy has given * as the definitive result for the longitude of the "Old Observatory" at Port Famine, which formerly served as the fundamental point for the surveys of Tierra del Fuego, $4^h\ 43^m\ 51^s{\cdot}5$ W. But if we take from Fitz-Roy's results only the difference measured between Monte Video and Port Famine—for which he assumes as the value mostly to be relied upon $58^m\ 58^s{\cdot}68$ according to the ten chronometers most nearly agreeing in the two runs constituting the connection, from Monte Video to Port Desire, and from Port Desire to Port Famine; which value, however, would be diminished by $2^s{\cdot}19$, using the mean of all the 17 and 16 chronometers respectively—and if we substitute for his own result for Monte Video the longitude as determined recently

* *Narrative of the Surveying Voyages of His Majesty's Ships "Adventure" and "Beagle,"* Appendix to vol. ii. p. 348.

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with much greater precision by telegraph,* this value ought to be increased to $4^h\ 43^m\ 55^s{\cdot}11$. This result, which would appear very trustworthy from the excellent harmony between the separate results of the great majority of the chronometers made use of, furthermore seems to receive a complete and strong corroboration from that derived by Capt. King from the operations of the preceding surveying expedition to Tierra del Fuego;† on the contrary, from the results of the German Transit of *Venus* Expedition 1882 IV., the true value appears to be nearly $4^h\ 43^m\ 45^s$, this number being subject to an uncertainty, amounting perhaps to 2^s, from the circumstance only that there remain some doubts concerning the connection between the German Observatory at Punta Arenas and the old observation spot at Port Famine, distant about thirty nautical miles.‡

There was, therefore, a discordance amounting to 10^s, which it seemed impossible to explain by accidental errors of the measurements, and which induced me to examine whether the old determination might be affected by systematic errors, and if possible to eliminate such errors. Before reporting upon this investigation, it seems necessary to refer to some details of the arrangement and the execution of the "Beagle's" measurements.

For her second voyage, begun at the end of the year 1831, the "Beagle" was equipped with twenty-two chronometers, which were placed on board and managed during the voyage with the utmost care, limits being imposed on the latter only by the necessity, inevitably connected with the circumstances of the undertaking, of having the chronometers kept going through a period of six years without being cleaned, and for a number of them by the necessary use in surveying operations.

Unfortunately no material of any kind has been given wherefrom to form a judgment of the absolute and relative accuracy of the different chronometers, all of which, of course, when delivered to the "Beagle," will have been considered to be superior instruments, except the separate values given by each chronometer for the several meridian distances, and a kind of classification made by Fitz-Roy, probably founded upon his general impression from the performance of the single chronometers during the voyage. The following list contains the statement of the class of instruments he considered them to be. Next to this I give the relative weights, differing materially in some instances from Fitz-Roy's classification, which I have been ultimately led to assume:—

* *Telegraphic Determination of Longitudes on the East Coast of South America in* 1878 *and* 1879. Washington, 1880.

† *Narrative*, vol. ii. App. p. 322; difference with Monte Video, $58^m\ 58^s{\cdot}07$, being the mean of 54 separate chronometrical results, which considered singly might appear less accurate than those of Fitz-Roy, but in the mean result will make this difference perhaps even more than good by reason of their being gathered in several, probably six or eight, voyages made in both directions.

‡ See Note following this paper.

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Box Chronometers.

Letter.	Maker and No.	Fitz-Roy's remarks.	Weight.
A	Molyneux 1415	good	4
B	Gardner 24	bad	$\frac{1}{4}$
C	Molyneux 1081	rather good	2
D	Murray 542	,,	1
E	Eiffe E	,,	$\frac{1}{2}$
F	Arnold and Dent 661	,,	$\frac{1}{2}$
G	,, ,, 633	,,	1
L	Arnold 634	,,	$\frac{3}{4}$
M	Frodsham 2	,,	$\frac{1}{2}$
N	Molyneux 1175	,,	$1\frac{1}{2}$
O	Earnshaw 705	,,	3
P	Frodsham 1	bad	$\frac{1}{2}$*
R	Murray 584	very good	3
S	Arnold 465	rather good	2†
W	Molyneux 971	good	$1\frac{1}{2}$
X	Earnshaw 509	rather good	$\frac{1}{2}$
Z	French 4214	good	4

Pocket Chronometers.

H	Arnold and Dent 261	rather good	$\frac{1}{2}$
K	Parkinson and Frodsham 1042	good	$\frac{1}{4}$‡
T	Molyneux 1326	indifferent	0
V	Pennington 426	rather good	$\frac{1}{2}$
Y	Morrice 6144	,,	$\frac{1}{2}$

With a view of easily distinguishing the pocket chronometers from the others, in what follows their letters are put in parentheses.

With these chronometers—strictly speaking with a number of them gradually diminishing during the course of the voyage—twenty-seven fundamental differences of meridians have been measured; the two first and the five last constituting a double connection between Bahia and England, and the remainder forming the twenty links of the celebrated "chain of longitudes" extending round the earth, proceeding westward from Bahia and returning to the same place. The first of these links, Bahia—Rio de Janeiro, was measured three times; because a large discordance between the result of the first measure-

* Appeared entitled to weight 1 but for Fitz-Roy's remark.
† In Section III. (see below); in the earlier parts of the voyage $p = \frac{1}{4}$.
‡ Employed regularly in the observations.

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ment with another made shortly before by Baron Roussin, immediately perceived by Fitz-Roy after the arrival at Rio, made an immediate control desirable, which was effected by sailing back at once for Bahia. All the other links of the chain depend on single measurements. The local times to be compared were always determined by equal altitudes of the sun taken with a very good sextant; the rates were determined by observations of the same kind, the stoppages at the different stations, in most instances, being long enough to allow the rates to be derived from a comparison of the first and last sets of equal altitudes with an accuracy fully adequate to the presumable accuracy of the transport of local time from one station to another; only towards the end of the voyage the stoppages sometimes appear rather too short. To carry the local time from one meridian to another, the two including determinations of rate have been made use of; but it is not stated whether the means of the two rates have been used, or the change applied exactly proportional to the time.* In the great majority of cases, however, both modes of computing would have led to very nearly the same results.

The only particulars of the measurements published in Fitz-Roy's Report† are: the spots where the observations for time were made at the different stations; the differences found, as the results of a computation remaining unknown as to all its details, between each two consecutive meridians by the single chronometers—wherein it is somewhat striking that in no single instance results of all the twenty-two chronometers are given; and the interval between the last observation for time at the preceding and the first at the following place. By means of the data contained in the "Abstract of the Meteorological Journal,"‡ and from occasional notices given in the narrative, the dates of these observations themselves can be made out within one or two days in most instances; and in every instance the exact times of the runs from one place to another which ought to serve as an essential element in the following investigation. From the Meteorological Abstract can be derived at least some continuous indications of the temperature of the chronometers during the several runs and ratings, as their temperature can be supposed, without fear of much error, except some few periods, to have nearly agreed with the temperature of the cabin barometer; but there remains the serious drawback, that the mean daily temperatures cannot be more than approximately estimated, on account of there being generally only one reading of the attached thermometer—and only one of the observations for the temperature of the air—given for each day; most of these observations.

* An express statement concerning the method adopted to allow for the rates, vol. ii. App., p. 330, appears to relate not to the determination of the fundamental differences, but to the insertion of secondary meridians.

† *Narrative*, vol. ii. App., pp. 331–344.

‡ *Ib.*, vol. ii. App., pp. 1–62.

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are taken during the warmer half of the day, but there is a rather large difference between the regular hours of observation in different periods of the cruise.

The following enumeration contains:—a summary of the measured differences of meridians and the designations by which they are referred to in the subsequent investigation; the (approximate) dates of the observations for time next including each single run; the actual duration of the run; and the approximate mean temperatures in which the chronometers were rated before and after the run, and in which they were kept between the two periods. The numbers given as such temperatures are the simple means of all readings of the attached thermometer given for the interval in question in the Meteorological Abstract for the hour of observation, regular for that period of the voyage; strictly speaking, therefore, the adopted numbers represent temperatures, which generally—except perhaps the first and the last measurement—exceed the actual mean temperature of the chronometers during the interval in a slight but slowly varying degree.

Besides these data, I give in the following summary other values of the longitudes of Fitz-Roy's stations in those instances where more certain determinations than those of Fitz-Roy himself—or equally certain ones in some few instances—exist, or have come within my knowledge.

a. Devonport to Porto Praya (S. Jago, Cape Verde Islands). Observations, 1831 Dec. 27, 1832 Jan. 16. Duration of the run, 20 days. Mean of observed temperatures: at Devonport, 52° (uncertain); during the run, 64°; at Porto Praya, 76°.

Place of observation at Devonport: The Baths, in the meridian of Government House, 1′ 48″·1 W. of Plymouth old church. The table of positions in the *Connaissance des Temps*, the only source wherefrom I can derive the longitude of this starting point, states Plymouth new church to be 6° 28′ 14″ W. of Paris,* value which appears to have originally been 4° 8′ 5″ W. of Greenwich. The old church being 14″·9 W. of the new church,† the longitude from which to reckon the "Beagle's" measurements is 4° 10′ 8″ = $16^m\ 40^s{\cdot}53$ W.

Place of observation at Porto Praya: landing place at the west side of Quail Island.

b. Porto Praya to Bahia. Observations 1832, Feb. 5, March 2. Run, 19 days. Temperatures, 76°, 80°, 82°.

Place of observation at Bahia: Fort S. Pedro. By the U. S. N. Expedition the longitude of the lighthouse at S. Antonio has been determined, in 1879, $=2^h\ 34^m\ 8^s{\cdot}37$. The relative situation of the two spots I can only take from a small

* This number depends on the Trigonometrical Survey of England and Wales, multiplying the meridional distances as computed by Mudge by the constant factor 1·003293.

† *Narrative*, vol. ii. App. p. 321.

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(1 : 150000) but carefully drawn plan, which makes Fitz-Roy's station $2^{s}\cdot95$ E. and $1'\ 30''\cdot6$ N. of S. Antonio.

1. Bahia—Rio de Janeiro. First measurement, 1832 March 15–April 6. Run, 17 days. Temperatures, 82°, 82°, 76°. Second measurement, in opposite direction, May 10–16. Run, 5·5 days. Temperatures, 76°, 76°, 80°. Third measurement, May 22–June 4. Run, 11 days. Temperatures, 80°, 76°, 74°.

Place of observation at Rio: Fort Villegagnon. Longitude determined by the U. S. N. Expedition $=2^{h}\ 52^{m}\ 37^{s}\cdot95$.

2. Rio de Janeiro to Monte Video. Observations, 1832 July 4, July 27. Run, 21 days. Temperatures, 74°, 66°, 58°.

Place of observation at Monte Video: Rat Island, according to King,* $1'\ 55''$ west of the Cathedral; according to Fleuriais,† $7^{s}\cdot16$. Taking the mean of these two values, from the U. S. N. Expedition's longitude of the S.E. tower of the Cathedral $=$ $3^{h}\ 44^{m}\ 49^{s}\cdot02$, the longitude of the spot so frequently employed by English vessels to rate their chronometers is : $3^{h}\ 44^{m}\ 56^{s}\cdot43$.

3. Monte Video to Port Desire (Spanish ruins). Observations, 1833 Dec. 5, Dec. 24. Run, 17 days. Temperatures, 69°:, 62°, 61°:.

4. Port Desire to Port Famine (old Observatory, at the west side of the port). Observations, 1834 Jan. 21, Feb. 5. Run, 11 days. Temperatures, 61°:, 59°, 54°. For the longitude of Port Famine see the note appended to this paper.

5. Port Famine (new Observatory, $5^{s}\cdot9$ E. of former place) to Ancud (S. Carlos, Chiloë, observation spot : Punta Arenas). Observations, 1834 June 7, July 3. Run, 19·5 days. Temperatures, 43°, 49°, 50°.

6. Ancud to Valparaiso. Observations, 1834 July 13, July 25. Run, 8 days. Temperatures, 50°, 55°, 56°.

Place of observation at Valparaiso: Fort S. Antonio, in the same meridian with the permanent point to which M. Fleuriais has referred (1868) his determination of the Cerro de la Cordillera. From some slight modifications of the results given by Gilliss and Moesta and a new reduction of the French observations‡ I find the most probable value of the longitude, giving equal weight to the four available determinations, $4^{h}\ 46^{m}\ 30^{s}\cdot84$.

For the difference between Ancud and Valparaiso the "Beagle's" voyage would afford another determination from the direct backward run in November 1834, which would appear to deserve equal confidence, but is not given.

7. Valparaiso to Callao. Observations, 1835 June 27, July 21. Run under sail, 15 days, rather long stoppages intervening at Coquimbo and Iquique. Temperatures, 58°, 63°, 65°.

Place of observation at Callao: the Arsenal. To the same meridian, within the limits of the probable errors in the observations for time, the telegraphic connection with M. Fleuriais'

* *Narrative*, vol. i. p. 479. † *Conn. d. T.* 1870, Addit. p. 47.
‡ *Astron. Nachrichten*, No. 2587.

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station Pisco, effected by Lieuts. Leclerc and Benazé,* appears to refer. Adding the difference as found by these officers $=3^m\ 43^s{\cdot}75$ to my result from M. Fleuriais' observations,† the longitude becomes $5^h\ 8^m\ 37^s{\cdot}17$.

8. Callao to Galapagos Islands, Chatham Island (landing place at the S.W. side of Stephen's Bay). Observations, 1835 Sept. 6, Sept. 18. Run, 9 days. Temperatures, 65°, 69°, 72°.

9. Galapagos Islands, Chatham Island, to Charles Island (landing place at the S.E. part of Post Office Bay). Run, 2 days. Temperature, 72°.

10. Charles Island to Otaheite, Point Venus. Observations, 1835 Oct. 16, Nov. 17. Run, 29·5 days. Temperatures, 72°, 77°, 80°.

The longitude of Point Venus is, according to Dr. Powalky's computation of two occultations observed by Cook 1773 and 1774, and the only available one of the two occultations observed 1824 by Preuss $= 9^h\ 57^m\ 56^s{\cdot}1$ W.‡ A determination of the longitude of Motu Uta, in the harbour of Papeete, made in 1859 by officers of the *Novara*,§ affords the following results:

	h	m	s
Longitude from 2 culminations of the Moon's first limb ...	9	58	11·29
" 3 " " second limb ...	9	58	19·22
Mean	9	58	15·25
Longitude from the occultation of *Regulus* (disapp.), 1859 Feb. 16	9	58	11·7

On the same small island was situated the Observatory of M. A. Kulczycki, the longitude of which has been found, "by long series of lunar culminations," 151° 54′ 30″ W. of Paris,‖ which should correspond to $9^h\ 58^m\ 17^s{\cdot}4$ W. of Greenwich. The observations of M. Kulczycki not being published, it is not possible to correct exactly some systematic error, which might be attributed to their computation as well as to the Paris reduction of M. Fleuriais' observations made about the same time; we might arrive at this correction approximately by assuming the difference of the equinoxes, to which the places of the Moon and of the stars have been referred in the computation, equal to its mean value for all M. Fleuriais' series $= 0^s{\cdot}025$, and multiplying this difference by a mean factor 26, whence the correction to the computed longitude becomes $+0^s{\cdot}65$.

M. Kulczycki's Observatory was situated $20^s{\cdot}00$ west of Point

* *Conn. d. T.* 1873, addit. p. 21.

† *Astr. Nachr.*, as before.

‡ *Astr. Nachr.* No. 1459.

§ *Reise der Oesterreichischen Fregatte Novara um die Erde. Nautisch-physikalischer Theil*, p. 29. The result of the occultation is there given as $9^h\ 58^m\ 13^s{\cdot}3$. This number ought to be diminished by (approximately) $1^s{\cdot}6$ from a more correct interpretation of the Greenwich results for the errors of the tables of the Moon.

‖ *Conn. d. T.* 1873, Add. p. 4.

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Venus. The Austrian determination may be transferred to this point without risk of sensible error with the same value of the difference; then the three results and their mean will be:—

	h	m	s
Occultations of Cook and Preuss	9	57	56·1
Novara	9	57	53·5
M. Kulczycki	9	57	58·0
Mean	9	57	55·9

11. Otaheite to Paihia (Bay of Islands, New Zealand). Observations, 1835 Nov. 25, Dec. 23. Run, 25 days. Temperatures, 80°, 75°, 69°.

12. Paihia to Sydney. Observations, 1835 Dec. 29, 1836 Jan. 17. Run, 14 days. Temperatures, 69°, 71°, 73°.

Place of observation at Sydney: Fort Macquarie. By transporting three chronometers to the Paramatta Observatory and back, Fitz-Roy found his place $52^s{\cdot}0$ east of the Observatory. The longitudes ultimately given for the latter (by Rümker* $10^h\ 4^m\ 6^s{\cdot}25$, by Wurm† $10^h\ 4^m\ 4^s{\cdot}1$) remain rather largely in error; but a pretty accurate determination can be derived from the position of the present Sydney Observatory, situated $2^s{\cdot}59$ west of Fort Macquarie. The most reliable determination of the longitude of this Observatory, up to the present time,‡ I consider $10^h\ 4^m\ 49^s{\cdot}9$ E., which I derived some years ago from all published lunar culminations observed at Sydney and Williamstown, numerous occultations observed at Windsor, and the longitudes determined by the German and English Expeditions for observing the Transit of *Venus* 1874 (Auckland Islands and New Zealand).

13. Sydney to Hobart Town. Observations, 1836 Jan. 29, Feb. 10. Run, 6 days. Temperatures, 73°, 67°, 67°.

Place of observation at Hobart Town: "East side of Sullivan Cove, in a small battery close to the water"—probably Prince of Wales Battery, next to the S.E. point of the bay, which opens towards E. Recently the longitude of Hobart Town has been determined by telegraph from Melbourne, but I do not know of a published result.

14. Hobart Town to King George Sound (Government buildings, at the east side of Princess Royal Harbour). Observations, 1836 Feb. 16, March 7. Run, 18 days. Temperatures, 67°, 64°, 68°.

15. King George Sound to Keeling Islands (N.W. part of

* *Phil. Trans.* 1829. part iii. p. 29. The difference, evidently between the same two spots, is there stated to have been measured by Rümker, by frequent transports of chronometers, $= 51^s{\cdot}93$.

† *Astr. Nachr.* No. 200.

‡ Only just when sending this paper to the press, I received the February number of the *Monthly Notices*, wherein the result of the new telegraphic determination is mentioned as being $10^h\ 4^m\ 48^s{\cdot}93$. From Mr. Ellery's last Annual Report, however, even this result appears to be still open to correction.

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Direction Islet). Observations, 1836 March 13, April 1. Run, 19 days. Temperatures, 68°, 74°, 82°.

16. Keeling Islands to the Mauritius. Observations, 1836 April 12, April 30. Run, 17 days. Temperatures, 82°, 81°, 79°.

Place of observation at Mauritius: Port Louis, battery on Cooper's Island (Fort George). An accurate value of the longitude is expected to result from Mr. Gill's chronometrical connections with Aden, 1874–5, which are not yet published.

17. Mauritius to Simon's Bay. Observations, 1836 May 9, June 2. Run, 22 days. Temperatures, 79°, 73°, 64°.

The place of observation (S.E. end of the dockyard at Simon's Bay), was found to be $10^s \cdot 9$ west of the Royal Observatory near Cape Town by three chronometers. The longitude of the Observatory is, according to Mr. Gill, $1^h\ 13^m\ 54^s \cdot 74$ E.*

18. Simon's Bay to St. Helena. Observations, 1836 June 19, July 8. Run, 19 days. Temperatures, 64°, 67°·5, 71°.

The observations were made at St. Helena in the meridian of the then existing Observatory, the longitude of which Johnson had found from a comparison of his lunar culminations with those observed at Greenwich, Cambridge, and the Cape, $22^m\ 54^s \cdot 6$ E.† This determination, however, which Johnson himself considered to give a rather too great E. longitude, might be improved by adding to the comparisons computed by Johnson those with Abo, Munich, Edinburgh, Makerstoun, Cracow, and Prague computed by Daussy in the *Conn. d. T.* for 1837, and by substituting corrected values for the longitudes of the places of comparison. Deriving only a single result from each observation made at St. Helena, with a proper weight according to the number of observations compared, I find, first using only the observations of those places which afford comparisons of both limbs, viz. Greenwich, Cambridge, Cape of Good Hope, Abo, and Munich :—

	m s
Long. from 35 St. Helena observations of the first limb	= 22 53·29 (weight = 19·67 obs.)
Long. from 5 St. Helena observations of the second limb	= 22 53·09 (weight = 2·83 obs.)
Mean	= 22 53·19 (weight = 9·91 obs.)

excluding all comparisons differing more than 15^s from these means. In a second computation, including the comparisons with Edinburgh, Makerstoun, Cracow, and Prague, which occur in the first two years (1830–1), but are confined to the first limb, the result from this limb becomes: longitude from 39 St. Helena observations of the first limb = $22^m\ 52^s \cdot 18$ (weight = 24·6 obs.), and the mean from both limbs = $22^m\ 52^s \cdot 63$ (weight = 10·16).

The longitude of St. Helena would become more accurately known from Johnson's observations by comparing them with

* *Monthly Notices*, vol. xliii. p. 418.

† Johnson, *A Catalogue of 606 Principal Fixed Stars*, Introd. p. 32.

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places of the Moon computed from Hansen's tables, because there remains a number of observations, and among these nine more observations of the second limb, for which no observations are available for comparison. For the present I take as the most probable result of Johnson's observations the mean of the above values $= 22^m\ 52^s{\cdot}91$. The *Conn. d. T.* has much diminished in later editions (since 1869) the longitude of St. Helena, and gives now the value $22^m\ 50^s{\cdot}9$, without referring to the authority for the new determination.

19. St. Helena to Ascension. Observations, 1836 July 14, July 20. Run, 6 days. Temperatures, 71°, 73°, 76°.

Place of observation at Ascension, centre of Barrack Square. Three occultations observed by Mr. Gill * at Mars Bay in 1877 would give for the same place, applying the difference $2^s{\cdot}07$ according to the Admiralty chart, $0^h\ 57^m\ 41^s{\cdot}45$ W., but this number is subject to a doubt of some seconds owing to the uncertainty of the positions of the Moon employed in the computation.

20. Ascension to Bahia. Observations, 1836 July 24, Aug. 2. Run, 9 days. Temperatures, 76°, 77°, 78°.

v. Bahia to Pernambuco. Observations, 1836 Aug. 6, Aug. 13. Run, 6·5 days. Temperatures, all equal, 78°.

The observation spot at Pernambuco, at the S.W. end of the Arsenal, appears to have been very nearly the same as that occupied by the American transit in 1878, in longitude $2^h\ 19^m\ 28^s{\cdot}96$ W.

w. Pernambuco to Porto Praya. Observations, 1836 Aug. 17, Aug. 31. Run, 13·5 days. Temperatures, 78°, 81°, 81°.

x. Porto Praya to Angra, Terceira. Observations, 1836 Sept. 5, Sept. 20. Run, 14·5 days. Temperatures, 81°, 78°, 75°. The observations were made at Angra, "close to the best landing place."

y. Angra to Devonport. Observations, 1836 Sept. 23, Oct. 7. Run, 9 days. Temperatures, 75°, 66°, 59°.

During this run the vessel stopped at Falmouth three days, observations for time being made at Pendennis Castle (longitude by *Conn. d. T.*, corrected from Trig. Survey, $20^m\ 10^s{\cdot}93$ W.).

z. Devonport to Greenwich. Observations, Oct. 17, Oct. 27. Run, 10 days. Temperatures, 59°, 60°, and, very uncertain, 53°.

On the run a comparison of the chronometers was made at the R.N. College, Portsmouth (longitude from *Conn. d. T.*, as before,† $4^m\ 24^s{\cdot}80$ W.).

The following table contains the differences of meridians as found by the single chronometers.

* *Mem. R.A.S.* vol. xlvi. p. 34.

† The *Nautical Almanac* appears still to retain the uncorrected longitude of the Trig. Survey, $4^m\ 23^s{\cdot}9$.

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Table I.

Meridian Distances.

Measured by Chron.	a	b	1¹	1²	1³	2	3	4	5	6
	1h 17m	0h 59m	0h 18m	0h 18m	0h 18m	0h 52m	0h 38m	0h 19m	0h 11m	0h 8m
	s	s	s	s	s	s	s	s	s	s
A	21·68	64·67	32·50	29·58	31·17	16·19	46·88	70·57	68·12	55·69
B	21·80	59·41	—	—	42·45	8·57	—	69·39	97·16	44·51
C	20·69	61·85	28·98	31·50	28·02	9·75	44·08	70·65	52·62	62·07
D	17·03	40·68	30·47	31·46	28·65	16·11	50·17	69·03	54·60	66·01
E	20·33	52·21	—	27·79	34·16	14·98	50·30	—	57·52	66·72
F	23·03	64·06	35·52	31·87	31·79	14·57	46·01	70·70	—	42·77
G	21·43	66·00	33·90	30·89	30·42	17·57	48·37	65·07	70·02	53·90
(H)	17·16	—	31·59	29·92	—	11·28	43·01	69·71	47·95	68·10
(K)	23·90	77·99	31·25	30·09	28·02	27·36	56·56	62·10	70·99	62·60
L	21·12	61·60	29·76	30·22	31·13	22·89	48·04	70·35	63·68	60·17
M	20·47	59·56	38·23	29·68	—	—	40·01	75·84	58·40	—
N	24·42	63·95	30·98	29·60	33·41	19·79	42·75	—	66·37	59·17
O	—	49·87	31·35	31·17	29·49	14·14	—	—	—	—
P	17·73	63·58	32·06	31·37	33·34	13·06	—	—	78·26	47·60
R	19·90	63·48	33·42	31·61	33·09	20·83	45·65	72·20	69·42	55·64
S	20·52	48·47	27·13	—	39·47	12·35	51·49	51·65	51·87	—
(T)	—	41·47	29·43	31·44	31·33	9·89	—	99·07	51·00	—
(V)	22·23	71·17	26·70	29·71	31·21	—	—	—	42·42	69·05
W	20·93	63·91	27·88	33·02	32·13	14·08	45·45	74·22	55·93	63·39
X	21·08	62·19	30·63	31·83	29·96	14·42	39·95	67·31	34·26	70·39
(Y)	20·58	62·47	38·79	32·57	27·55	40·60	27·14	—	55·13	61·49
Z	21·43	64·69	31·51	31·18	30·91	18·60	47·32	70·29	61·42	58·34

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	7	8	9	10	11	12	13	14	15	16
Measured by Chron.	$0^h\ 22^m$	$0^h\ 49^m$	$0^h\ 3^m$	$3^h\ 56^m$	$2^h\ 25^m$	$1^h\ 31^m$	$0^h\ 15^m$	$1^h\ 57^m$	$1^h\ 23^m$	$2^h\ 37^m$
	s	s	s	s	s	s	s	s	s	s
A	7·74	31·80	39·49	11·67	38·69	33·50	29·40	48·75	67·62	38·96
B	0·51	32·30	37·29	7·07	37·50	27·63	26·30	26·50	67·17	36·30
C	7·31	33·90	39·11	7·53	32·87	33·64	34·31	59·28	69·15	35·05
D	6·86	33·49	39·23	5·43	35·11	31·84	35·28	63·90	69·16	31·87
E	4·42	—	—	—	—	—	—	—	—	—
F	16·60	—	—	—	—	—	—	—	—	—
G	5·90	—	44·81	—	33·99	27·41	30·96	54·15	72·50	17·34
(H)	—	—	—	14·04	35·66	23·94	—	54·31	65·60	—
(K)	—	30·39	36·67	—	38·20	44·60	29·86	42·94	63·09	48·06
L	—	—	38·23	20·65	27·70	32·09	30·91	55·21	83·16	43·18
N	—	36·74	41·69	14·57	40·78	32·52	30·83	57·77	68·64	33·17
O	8·66	33·15	39·36	14·19	34·97	29·29	29·25	51·26	67·72	32·54
P	8·97	—	—	—	—	—	—	—	—	—
R	11·33	32·16	39·40	14·91	36·68	30·69	26·17	42·67	69·53	37·62
S	11·39	32·56	39·44	11·35	28·83	30·17	32·01	52·67	66·55	31·94
(V)	—	—	—	—	27·99	—	38·84	—	—	25·55
W	12·28	35·21	39·19	8·27	32·83	28·52	25·48	36·04	60·06	29·81
X	3·30	29·44	39·28	9·35	28·89	26·82	30·41	47·47	43·24	27·70
Z	9·36	32·90	39·52	16·74	40·48	32·36	32·31	50·96	67·44	34·42

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Measured by Chron.	17 2h 36m	18 1h 36m	19 0h 34m	20 1h 36m	v 0h 14m	w 0h 45m	x 0h 14m	y* 1h 32m	z† 0h 16m
	s	s	s	s	s	s	s	s	s
A	25·12	38·39	45·95	27·18	35·82	23·77	50·08	9·76	34·9
B	—	—	44·18	20·25	37·84	28·32	49·39	—	—
C	18·23	31·03	44·96	25·75	36·03	27·40	48·93	8·60	37·8
D	28·16	29·82	45·00	28·31	34·89	27·29	51·46	10·62	42·7
G	32·50	31·46	45·72	24·47	36·80	28·04	52·59	7·15	42·2
(H)	—	—	44·15	23·48	36·76	28 71	50·39	13·06	—
(K)	24·85	43·37	45·42	22·36	37·41	22·70	49·20	—	—
L	23·62	30·34	48·23	28·76	34·85	30·95	51·43	8·82	39·7
N	24·93	27·63	46·37	29·92	35·02	25·03	49·26	8·95	42·1
O	23·46	32·14	45·26	23·56	36·23	28·89	49·88	7·35	43·4
R	21·44	37·70	46·37	26·65	36·19	—	—	—	—
S	18·82	31·00	45·72	24·71	36·79	28·38	48·52	10·29	38·1
(V)	—	29·90	—	—	37·80	29·40	—	10·33	45·5
W	17·74	37·24	45·93	26·28	35·24	27·46	50·43	—	—
X	12·44	—	—	32·50	—	—	—	—	—
Z	19·68	33·96	46·22	26·12	36·97	26·24	50·88	9·52	36·3

* Included: Angra to Falmouth:—

	h	m	s		h	m	s
Chr. A	1	28	38·61	Chr. N	1	28	40·07
C			39·92	O			38·64
D			41·61	S			41·49
G			37·11	(V)			40·80
(H)			42·99	Z			38·15
L			38·43				

† Included: Portsmouth to Greenwich:—

	h	m	s		h	m	s
Chr. A	0	4	22·35	Chr. L	0	4	24·07
B			24·57	N			26·63
C			22·63	O			29·17
D			26·73	S			23·29
G			26·46	Z			21·81
(H)			29·79				

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By a glance at the foregoing table it is seen that only by four of the chronometers (A, C, D, Z) were all the meridian distances measured, or by five, if S is added to this number, which is missing only in the middle one of the three runs between Bahia and Rio; besides, unbroken chains are presented by W until back to Porto Praya, and by R as far as Pernambuco. Moreover, the measurements by G, L, and N extend from the beginning to the end, but in each of these chains two links are wanting. All the other chains are broken by a greater number of gaps, or extend altogether only from England to the west coast of America. In many instances we cannot but suppose the wanting result to have been suppressed, having been considered untrustworthy on account of its differing from the mean of the others beyond such limits as appeared tolerable, or for too great differences between the rates obtained before and after the run. The great number of breaks in the chains measured by B, (H), (V), and X ought probably to be generally explained in this manner, allowance for which ought to be made, if the relative accuracy of the chronometers is to be judged by the deviations from true or most probable values of the meridional distances, of the separate results *which are given* by Fitz-Roy. On the contrary, that chronometer O is not mentioned in the connection of Monte Video with Valparaiso, evidently comes from quite another circumstance; there cannot be doubt that this chronometer, considered by Fitz-Roy only as rather good (according to my computations one of the very best), has been during that period—and so probably (V) too, in the runs from Port Famine to Valparaiso—on board the tender "Adventure" ("Unicorn"). Of the six chronometers, E, F, M, P, (T), and (Y), entirely wanting in the measurements beyond Callao, four—doubtless E, F, M, and (Y)—were left behind for the completion of the survey of the Peruvian coast by the tender "Constitucion." P is expressly stated not to have been used, because of irregularities of its rate, after September 1835 (from the measurements made with this chronometer, which are given in Fitz-Roy's report, it would appear, however, to have been quite a good instrument), (T) appearing to have been considered as a useless watch since an earlier date. As to L and N lacking in the measurement 7, Valparaiso to Callao, I suppose them to have made this run on board the "Constitucion," instead of E and F, which remained on board the "Beagle" till arrival at Callao. If this supposition is right, there would be in each one of the chains L and N only one measurement, which might appear to have been suppressed for too great a discordance.

Fitz-Roy has adopted as final result for each difference of meridians the simple arithmetical mean from the ten chronometers which gave the most accordant results *inter se* for the difference in question, entirely excluding all the remaining individual results, in some instances the majority of them all. Such a proceeding may give tolerably approximate results in the simplest manner, but

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besides being likely to produce, and apparently having produced in this case, illusory ideas on the accuracy of the mean results, it will certainly not afford the most probable values of the single differences; but it cannot by any means be admitted to connect them into a chain, if so doing leads to the consequence that the different links will depend partly on different chronometers, and if at the same time the errors of the chronometrical measurements cannot be regarded as purely accidental; both of which happen to be the case in the present instance.

As a consequence of his method of forming the results for the several links to be joined together, Fitz-Roy has excluded from some meridian distances the very best chronometers, admitting the worst ones with full weight, although these had been excluded for the great majority of the distances. As to systematic errors of the measurements, it ought to be remembered that the great chain Bahia—Bahia proved to be in error by no less than 34^s, an amount the possibility of which Fitz-Roy found himself quite at a loss to understand, as he could not imagine that so large an error could by any means have been produced by the accidental errors of the determinations of local times and of rates, or by the accidental changes of the latter. Granted that Fitz-Roy himself has perhaps considerably underrated the possible effect of these sources of errors, in consequence of dealing only with a selected number of the individual results, they cannot have been in reality so large that any increase, which might be expected from the probability of accidental errors in adding together the twenty links of the chain, could produce a total error comparable with the amount actually found, and Fitz-Roy is very nearly right in saying "it does not appear probable that all the errors would lie in one direction, unless some hitherto undetected cause affects chronometers when carried westward, which might affect them differently when carried eastward." *

Finally, Fitz-Roy has derived the results from his measurements in the following way. To connect Bahia with England, he took the mean of the two results obtained from the outward and the homeward voyage, which were in fair accordance, apparently directly denying the existence of any such "undetected cause," and thereby rendering matters still more unaccountable. From Bahia he simply summed up the measured differences without allowance for the error of the total sum westward to the Galapagos Islands and eastward to New Zealand. Thus the whole amount of error is thrown upon the passage over the breadth of the Pacific, and becomes evident in two values of the longitude of Point Venus being obtained, differing from each other 34^s, of which Fitz-Roy took the mean. How the Hydrographical Office dealt with the error, whether it has tried to arrive at a more reasonable repartition of the whole

* *Narrative*, vol. ii. App. p. 345.

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amount, I have no knowledge. In drawing up the French table of positions, Daussy has retained unchanged the longitudes as summed up by Fitz-Roy westward to the Galapagos Islands, and has abstained from making use of the remaining portion of the chain, besides combining for some few eastern places Fitz-Roy's differences with other available determinations. In late editions of the table, the longitudes given by Fitz-Roy along the South American coast have been shifted in groups, according to new French determinations of some of the fundamental meridians.

There is, however, existing an obvious cause which—while affecting the rate of a chronometer, remaining on board in a permanent position, of course always in the same direction—would affect a chronometrical measurement of a difference of meridians in one direction when the chronometer is carried to the west, and in the opposite direction when it is carried towards east. Such a cause is the effect of the motion of the ship. The changes of rates produced by this cause in the case under consideration are to be estimated as much smaller than the changes found in some modern runs with chronometers made by steamships; but we should be very far from right in considering them altogether to be inappreciable in sailing vessels, because the motion of such a vessel also does not consist merely in such vibrations of which the chronometers become independent when swinging freely in their gimbals, but the vessel is also exposed to concussions which will generally communicate themselves to the chronometers—the more so in a small vessel like the "Beagle," measuring only 235 tons—notwithstanding all care in a proper selection and arrangement of their situation on board. It appears at once from a glance at the preceding table, that the better chronometers very regularly differ from each other in the same direction in the measurements of the separate distances, thus directly pointing to regular discordances between sea-rates and harbour-rates of an amount materially different in the different chronometers. For example, A, out of 29 differences of meridians, gives 21 greater (taking the positive sign for western longitudes) than C; O, 15 out of 20 smaller than R; W, 19 out of 26 smaller than Z. Distributing the excess, $33^s{\cdot}67$, of the great chain Bahia—Bahia, by applying simply a correction of $-0^s{\cdot}115 \times$ duration of run (expressed in days) to the results adopted by Fitz-Roy, entirely disregarding for the moment their want of homogeneity, I arrived at the following comparison for the places where the "Beagle's" measurements can now be tested by other determinations:—

Place.	Longitude summing up Fitz-Roy's results westward.	Error.	Correction $-0^s{\cdot}115t$.	Remaining Error.
	h m s	s	s	s
Bahia	2 34 5·42		The starting value	
Rio de Janeiro ...	2 52 36·93	− 1·02	− 0·86	−1·9
Monte Video ...	3 44 54·53	− 1·90	− 3·27	−5·2

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Place.	Longitude summing up Fitz-Roy's results westward.	Error.	Correction $-0^{s}\cdot115l$.	Remaining Error.
	h m s	s	s	s
Port Famine ...	4 43 53·21	+ 7·91	− 6·50	+1·4
Valparaiso ...	4 46 47·24	+16·40	− 9·65	+6·7
Callao	5 8 56·22	+19·05	−11·38	+7·7
Otaheite	9 58 20·75	+24·85	−16·03	+8·8
Sydney	13 55 27·86	+20·36	−20·52	−0·2
Simon's Bay ...	22 46 53·43	+37·27	−29·95	+7·3
St. Helena ...	0 23 26·74	+33·83	−32·13	+1·7
Bahia	2 34 39·09	+33·67	−33·86	−0·2

The large reduction of errors resulting in all the available comparisons, from so superficial an attempt to correct the measurements as reduced by Fitz-Roy, certainly ought to be regarded as an encouragement to a more exact proceeding, in which the measurements made with each single chronometer were to be treated separately.

As a basis for the necessary corrections, I again took the chain Bahia—Bahia. The measurement of this chain is complete with six chronometers, and with a seventh the chain extends unbroken from Bahia to the Cape of Good Hope. The excess for the first six over 24 hours in 294·5 days of running,* for the last, X, over the true value of the arc Bahia—Simon's Bay in 260·5 days, is as follows :—

A + 55·51	R + 52·59	X − 81·64
C + 10·75	W − 13·72	
D + 31·65	Z + 47·06	

These excesses were distributed among the several links of the chains proportionally to the intervals,† and preliminary means taken for the single links, assuming such relative weights as might be inferred from Fitz-Roy's classification of the chronometers (the pocket chronometers, in the subsequent extension of this operation, being placed in the class next below).‡ After

* For difference 1 the mean of the three measurements has been made use of; in computing the corresponding mean duration of run, the time employed in the second run, made in the direction opposite to that in the first and third runs, ought to be reckoned as negative.

† By inadvertence, proportionally to the intervals given by Fitz-Roy, instead of the mere durations of run, without subtracting from the former the time elapsed between the including observations and the beginning and end of the run respectively. This error, however, cannot have exercised any material influence upon the results.

‡ The weights adopted in this part of the investigation have been :

4 for chr. R
3 for chr. A, W, Z
2 for chr. (K); C, D, E, F, G, L, M, N, O, S
1 for chr. (H), (V), (Y)
0 for chr. (T); B, P.

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this it became possible to supply the missing link in chain S, and the two links missing in each one of the chains G, (K), L, and N, by inserting such values as would differ from the mentioned preliminary means by quantities proportionate to the differences in the two or three next preceding and as many of the next following measurements. The excesses of the chains filled up in this manner were then found to be for

G +44·20	L+73·08	S−15·46
(K)+67·89	N+63·68	

and were distributed as before. A comparison with a new series of preliminary means formed with the twelve chronometers then afforded the means to annex those chains which were broken by a greater number of gaps, after which definitive means were formed within the reach of the great chain.

Finally, the reductions derived from this chain were used to correct the distances measured from and to England, and, the measurements extending to Greenwich, the longitudes of all the places of observation became determined.

The result of this operation, however, then proved to be unsatisfactory to a most unexpected amount. The outstanding errors for the places of comparison were:—

	s		s
Devonport	+0·1	Valparaiso	+0·7
Pernambuco ...	−1·6	Callao	+1·3
Bahia	−3·1	Otaheite	+4·0
Rio de Janeiro ...	−5·0	Sydney	−4·8
Monte Video ...	−8·8	Simon's Bay ...	+3·9
Port Famine ...	−5·0	St. Helena	−1·0

The summing up of the defect to a large amount on the relatively short first track is intolerable, and still more so the rapid change of the error in the short run from Monte Video to Valparaiso; besides, the two results for Bahia coming out 8s different, there now appeared anything but the harmony Fitz-Roy had found—more accidentally, however, for the mean than actually for the separate chronometers—in the two connections with England.

The impression created by the foregoing table of errors was that the measured meridian distances naturally divided into three groups; the points of separation being Monte Video and Valparaiso, the systematic changes in the rates of the chronometers totally differing from one group to another. After remarking this I searched the "Narrative" for an indication of circumstances which might have caused such a different performance, and then the measurements appeared, as is seen from the dates belonging to each one of them already tabulated above, which are not given in Fitz-Roy's table of the results, actually to

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divide in exactly those three sections, separated from each other by long intervals of time, and therefore necessarily to be treated separately. In Section I., Devonport to Monte Video, the measurements have been made in quick succession, very nearly during the first seven months of the year 1832. Then an interruption occurred, extending over almost a year and a half, during which period the oil in the chronometers necessarily changed considerably, and most probably their internal constitution altogether was sensibly altered under the influence of an almost incessant rough shaking, partly in the boisterous sea surrounding Falkland Islands and Tierra del Fuego, partly on board little craft on the worst part of the Patagonian coast. It ought not to be wondered at, therefore, if the chronometers, when the measurements of meridian distances for the chain were resumed in December 1833, were quite different instruments than before, and became influenced by the motion of the ship quite differently than in the voyage from England to Monte Video. Section II., which herewith is to begin, again extends over seven months, comprising only the short run from Monte Video to Valparaiso, but the measurement was interrupted for several months (February to June 1834) by surveying work. After this, again, there occurred an interruption of nearly one year, during which time it is quite reasonable to assume the change in the internal state of the chronometers would have considerably increased; therefore Section III., which extends from the end of June 1835 to the end of October 1836, and contains the measurements carried from Valparaiso at one draught—excepting a somewhat prolonged stay at Callao—to their end at Greenwich, embracing nineteen of the twenty-seven fundamental meridian distances, ought again to be treated separately.

In Section I. the differences

	h m s	h m s	h m s
$a+b$ =	2 17 24·89,	1 = 0 18 32·53,	2 = 0 52 18·48*

can be regarded as accurately known. By comparison with these numbers we get therefore several—for most of the chronometers five—equations. How to deduce from them the most probable value of the unknown quantity of the case, the mean difference between sea-rate and harbour-rate, depends upon the assumptions we may make as to the relative amount, in the total differences in question, of the errors of rating, the accidental or systematic variations of the actual value of the mentioned difference, and the accidental changes of rate produced by other causes. I took a middle course, at the same time affording greatest simplicity of computation, between the extreme assumptions which can be made in this respect, simply summing together the

* Instead of this number, $0^h\ 52^m\ 18^s{\cdot}21$ has been used in the computations, assuming Fleuriais' reduction to Rat Island alone, or longitude of observation spot = $3^h\ 44^m\ 56^s{\cdot}18$, and taking for this value, inadvertently, $56^s{\cdot}16$.

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excesses of the measured differences over the three known values of meridian distances—with due regard, of course, to the negative sign of measurement 1[2]—and dividing the sum by the sum of the durations of run. In this way the following numbers have been derived:—

Excess of Mean Daily Losing Rate (r_s), the ship being under sail in open sea, over Mean Daily Rate in harbour (r_h), for Section I. (meas. a, b, 1, 2)

	s		s		s		s
A	−0·011	G	−0·030	O	+0·127	(V)	−0·058
B	+0·049	(H)	−0·121	P	+0·077	W	+0·093
C	+0·191	(K)	−0·247	R	−0·037	X	+0·098
D	+0·148*	L	−0·007	S	+0·230	(Y)	−0·340
E	−0·041*	M	−0·060	(T)	+0·212	Z	−0·004
F	−0·016	N	−0·078				

It ought to be remarked that the variations of rate in the pocket chronometers scarcely exceed those resulting for many of the box chronometers.

In Section II. the following numbers have been derived for the twelve chronometers used in all measurements belonging to this section, by distributing the error of the whole line† Monte Video—Valparaiso over the 55·5 days of sailing. For the remaining nine chronometers the corresponding numbers have been found by comparing the distances as measured by them with such means as could be formed from the reduced measurements with the twelve chronometers, using preliminary weights, which shall be given hereafter.

Values of ($r_s - r_h$) in Section II. (meas. 3, 4, 5, 6)

	s		s		s	
A	−0·544	(H)	+0·130	S	+0·115	
B	−1·650 (3 dist.)	(K)	−0·493	(T)	−0·970	very uncertain, 2 dist.
C	−0·100	L	−0·401	(V)	+0·705 (2 dist.)	
D	−0·146	M	−0·265 (3 dist.)	W	−0·225	
E	−0·161 (3 dist.)	N	−0·360 (3 dist.)	X	+0·515	
F	−0·804 (3 dist.)	P	−1·378 (2 dist.)	(Y)	+0·296 (3 dist.)	
G	−0·538	R	−0·575	Z	−0·378	

Some of these values would in any case appear extraordinary for properly mounted box chronometers transported by a sailing

* Allowing for a disturbance, which these two chronometers appear to have suffered in meas. b.

† Assuming the longitude of Rat Island as in Section I., and Valparaiso 4[h] 46[m] 29[s]·96. I had not then for this place the result of my new reduction of Fleuriais' observations, by combining which with the other available determinations the longitude was considerably increased.

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vessel, and the more so when compared with the values occurring in the two other sections. The two greatest values, however, for B and P, are probably in reality chiefly accidental effects of irregular disturbances of rate, by which these two chronometers generally appear to have been affected beyond the average. The same explanation probably holds good for the greatest value next to these, for F, which results mainly from meas. 6; the value given by this chronometer for the preceding meridian distance is nowhere given, whence it ought to be supposed that in the run from Port Famine to Valparaiso its rate was irregular and different from what it was half a year before, when the distances constituting the first half of Section II. were run. The differences $r_s - r_A$ found for the other chronometers I do not consider to be so great that they might not be supposed to originate from the assumed cause; possibly other causes —for example, the variations of temperature—have contributed to them, but that the supposed systematic differences $r_s - r_A$ really have amounted to larger quantities in this section is by no means improbable from the motion of the ship probably being heavier than usual in the other runs; and the very satisfactory agreement of the determination of the middle place, Port Famine, with the true longitude, which will be obtained from the adopted supposition concerning the principal cause of the changes in the rates, is a further strong argument in favour of this supposition.

For the reduction of the last series of the measurements have been employed the following

Values of $r_s - r$ in Section III. (meas. of diff. 7–20, v–s).

A	−0·1517	Valparaiso* − Greenwich
B	+0·1058	Comp. with prel. means, excl. diff. 14
C	−0·0802	Valparaiso − Greenwich
D	−0·1114	„ „
G	−0·2166	Sydney − Greenwich, excl. diff. 16
(H)	+0·0098	Comp. with prel. means
(K)	−0·1721	Sydney − Pernambuco
L	−0·2065	Chatham Island† − Greenwich
N	−0·1624	Callao† − Greenwich
O	−0·0333	Valparaiso − Greenwich
R	−0·0836	Valparaiso − Pernambuco
S	−0·0080	Valparaiso − Greenwich

* Longitude always as before, $4^h\ 46^m\ 29^s{\cdot}96$. Allowance for the difference of this number from the most probable value will be made, where necessary, hereafter.

† Using for the longitude of the starting point preliminary means of the results from the other chronometers.

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(V)	+0.s1479	Comp. with prel. means
W	+0.0891	Valparaiso—Pernambuco
X	+0.1867	Valparaiso—Simon's Bay, excl. diff. 15.
Z	−0.1299	Valparaiso—Greenwich

The results by E, F, P belonging to this section cannot be used for lack of elements for their reduction, being confined to the first distance only.

The investigation, described above, comprising at once the whole extent of the voyage, had afforded a means to determine the relative accuracy of the different chronometers in a more approximate manner. Comparing the reduced individual measurements with the final results of that investigation I was led to the following classification, differing in some instances from what was previously assumed:—

Weight 4 : Chr. Z
„ 3 : Chr. A, O, N
„ 2 : Chr. C, R
„ 1 : Chr. D, E, G, (H), L, M, S, W

With these weights the "preliminary means" mentioned in Section II. and III., and used for the purpose of determining the values of $r_s - r_h$ for the broken chains, have been deduced.

The weights could be more accurately determined by comparing those meridian distances which had been reduced with the three separate systems of values $r_s - r_h$, with their preliminary means. Excluding some few cases of larger discordances—for certain reasons in each case, but which would be too long to explain in detail—I found for the average value of the probable error of a meridian distance $= \epsilon_1''$, and the corresponding weight p_1 (= 1 for the square of the mean error = 10), the following values, according to which the chronometers would range as below:—

Chron.	ϵ_1''	p_1	Chron.	ϵ_1''	p_1	Chron.	ϵ_1''	p_1
	s			s			s	
O	±1.0	4.15	D	±2.3	0.89	E	±3.1	0.47
Z	1.2	3.00	W	2.3	0.82	M	3.5	0.37
S_2	1.3	2.72	L	2.4	0.78	(K)	4.3	0.22
A	1.7	1.56	G	2.5	0.76	B	5.2	0.17
C	1.9	1.30	X	2.9	0.53	(Y)	6.0	0.13
N	1.9	1.21	(H)	3.0	0.52	$S_{1,2}$	6.3	0.11
P	2.0	1.08	F	3.0	0.49	(T)	10.9	0.04
R	2.2	0.93	(V)	3.1	0.49			

Chronometer S exhibits a great difference in its performance during Section I. and II., and during Section III.; during the first two its rate was very irregular, while in the last it agrees in an admirable manner with the mean of all. To explain this difference we may suppose, that during one of the stays at Valparaiso between Section II. and III. some error of the chronometer was detected and corrected; therefore, it appears

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legitimate to use it with different weights in the different periods.

Chronometer O becomes the first of all, when judged from the agreement with the mean of all chronometers, after excluding its result for difference b. I consider this proceeding legitimate, because from the discordance of this result the sum of the squares of errors for the remaining twenty-three differences (43·30) would be more than doubled, rising to 131·10, and because, moreover, the result for the first distance, a, is not given at all, and therefore cannot but be supposed to have come out quite wrong. Hence it becomes highly probable that at the beginning of the voyage, as far as Porto Praya, some extraordinary cause of error, perhaps only some defect in the mounting, influenced this chronometer, which was then noticed and removed.

The weights of the above table, however, cannot be admitted without further discussion: first, because of their want of homogeneity for the broken chains previously referred to, and secondly, because the relative weights deduced from the above absolute numbers would be erroneous, if there existed sources of error varying from one run to another, of such a nature as would affect the rates of a great number of the chronometers during the same run in the same way. There will be found, indeed, strong indications that a part of the actual errors is due to such causes. I therefore classified the chronometers by the constancy of the observed differences between sea-rates and harbour-rates. This can only be judged from Section III. Dividing the line Valparaiso—Greenwich at four points, viz.: Otaheite, Sydney, Cape of Good Hope, and Bahia, I obtained five determinations of this difference for eight of the twelve chronometers, on which the measurements essentially depend, and four determinations for three of the remaining four, from which resulted the following values of ϵ_2'', or of the probable error of 50 $(r_s - r_h)$, and corresponding weights p_2 ($p_2 = 1$ for a mean error $= \pm 10^s$):

Chron.	ϵ_2'' s	p_2	Chron.	ϵ_2'' s	p_2	Chron.	ϵ_2'' s	p_2
A	±3·4	4·0	O	±6·5	1·1	G	±8·0	0·7
Z	4·0	2·8	W	6·6	1·1	D	9·3	0·5
R	4·1	2·7	N	7·1	0·9	L	12·0	0·3
C	5·9	1·3	S_2	7·3	0·9			

The definitive weights already tabulated at the beginning of this paper, expressed in such round numbers as appeared most plausible, are founded essentially on these two determinations. The better chronometers range according to these weights in the following order: Z, A, O, R, S (in Section III.), C, N, W, G, D, L. Then follow the chronometers used only in a part of the measurements, not differing much from each other in quality, excepting, perhaps, the first of them: P, X, (H), F, (V), E, M; then come (K), B, finally (Y); (T) ought to be rejected.

The meridian distances resulting from the reduction of the measurements by the adopted values of $r_s - r_h$ are contained in the following table:—

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TABLE II.

Meridian Distances, corrected for Systematic Change of Rate.

	a	b	1¹	1²	1³	2	3	4	5	6
Measured by Chron.	1ʰ 17ᵐ	0ʰ 59ᵐ	0ʰ 18ᵐ	0ʰ 18ᵐ	0ʰ 18ᵐ	0ʰ 52ᵐ	0ʰ 38ᵐ	0ʰ 19ᵐ	0ʰ 11ᵐ	0ʰ 8ᵐ
	s	s	s	s	s	s	s	s	s	s
A	21·46	64·46	32·31	29·64	31·05	15·96	37·63	64·59	51·61	60·04
B	22·78	60·34	—	—	42·99	9·60	—	51·24	59·08	57·71
C	24·51	65·46	32·23	30·45	30·12	13·76	42·38	69·55	44·77	62·87
D	20·09	(43·59)	33·07	30·62	30·33	19·32	47·69	67·42	45·85	67·18
E	19·63	(51·54)	—	27·98	33·78	14·25	47·56	—	48·48	68·11
F	22·71	63·76	35·25	31·96	31·61	14·23	32·44	61·86	—	49·20
G	20·83	65·43	33·39	31·05	30·09	16·94	39·22	59·15	53·63	58·21
(H)	14·74	—	29·53	30·59	—	8·74	45·22	71·14	44·59	67·06
(K)	18·96	73·30	27·05	31·45	25·30	22·17	48·18	56·68	55·48	66·54
L	20·98	61·47	29·64	30·26	31·04	22·74	41·22	65·94	49·96	63·38
M	19·27	58·42	37·21	30·01	—	—	35·51	72·93	47·33	—
N	22·86	62·47	29·95	30·03	32·55	18·15	36·63	—	53·45	62·05
O	—	(52·28)	33·51	30·47	30·89	16·81	—	—	—	—
P	19·27	65·04	33 37	30·95	34·19	14·68	—	—	45·49	58·62
R	19·16	62·78	32·79	31·81	32·68	20·05	35·87	65·88	52·31	60·24
S	25·12	52·84	31·04	—	42 00	17·18	53·87	53·19	48·70	—
(T)	—	45·50	33·03	30·28	33·66	14·34	—	88·40	26·18	—
(V)	21·07	70·07	25·71	30·03	30·57	—	—	—	50·27	63·41
W	22·79	65·68	29·46	32·51	33·15	16·13	41·63	71·74	45·64	65·19
X	23·04	64·05	32·30	31·29	31·04	16·48	48·70	72·98	38·40	66·27
(Y)	13·78	56·01	33·01	33·94	23·81	33·46	32·17	—	55·00	59·12
Z	21·35	64·61	31·44	31·20	30·87	18·52	40·89	66·13	48·15	61·37

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Measured by Chron.	7	8	9	10	11	12	13	14	15	16
	h 22m	h 49m	h 3m	h 56m	h 25m	h 31m	h 15m	h 57m	h 23m	h 37m
	s	s	s	s	s	s	s	s	s	s
A	5·46	30·43	39·19	7·20	34·92	31·38	28·49	46·02	64·74	36·38
B	2·10	33·25	37·50	10·19	40·14	29·11	26·94	(28·40)	69·18	38·10
C	6·11	33·18	38·95	5·17	30·87	32·52	33·83	57·84	67·63	33·69
D	5·19	32·49	39·01	2·14	32·32	30·28	34·61	61·89	67·04	29·98
E	....	—	—	—	—	—	—	—	—	—
F	....	—	—	—	—	—	—	—	—	—
G	2·65	—	44·37	—	28·58	24·38	29·46	50·25	68·39	(13·66)
(H)	—	—	—	14·33	35·90	24·08	—	54·49	65·79	—
(K)	—	28·84	36·33	—	33·90	42·19	28·83	39·84	59·82	45·13
L	—	—	37·82	14·56	22·54	29·20	29·67	51·49	79·24	39·67
N	—	35·28	41·37	9·78	36·72	30·25	29·86	54·85	65·75	30·41
O	8·16	32·85	39·30	13·20	34·14	28·83	29·05	50·66	67·09	31·97
P	....	—	—	—	—	—	—	—	—	—
R	10·08	31·41	39·23	12·44	34·59	29·52	25·67	41·17	67·94	36·20
S	11·27	32·49	39·42	11·11	28·63	30·06	31·96	52·53	66·40	31·80
(V)	—	—	—	—	31·69	—	39·72	—	—	28·06
W	13·62	36·01	39·37	10·90	35·06	29·77	26·02	37·64	61·75	31·32
X	6·11	31·13	39·65	14·89	33·59	29·44	31·54	50·85	(46·81)	30·89
Z	7·41	31·73	39·26	12·91	37·23	30·55	31·53	48·62	64·97	32·21

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	17	18	19	20	v	w	x	y	z
Measured by Chron.	$2^h\ 36^m$	$1^h\ 36^m$	$0^h\ 34^m$	$1^h\ 36^m$	$0^h\ 14^m$	$0^h\ 45^m$	$0^h\ 14^m$	$1^h\ 32^m$	$0^h\ 16^m$
	s	s	s	s	s	s	s	s	s
A	21·78	35·51	45·04	25·81	36·81	25·82	47·88	11·13	36·42
B	—	—	44·82	21·20	37·15	26·89	50·92	—	—
C	16·47	29·51	44·48	25·03	36·55	28·48	47·77	9·32	38·60
D	25·71	27·70	44·33	27·31	35·61	28·79	49·84	11·62	43·81
G	27·74	27·35	44·22	22·52	38·21	30·97	49·45	9·10	44·36
(H)	—	—	44·21	23·57	36·70	28·58	50·53	12·96	—
(K)	21·06	40·10	44·39	20·81	38·53	25·02	46·70	—	—
L	19·07	26·42	46·99	26·90	36·19	33·74	48·44	10·68	41·77
N	21·36	24·54	45·40	28·46	36·08	27·22	46·91	10·41	43·72
O	22·73	31·50	45·06	23·26	36·45	29·34	49·40	7·62	43·73
R	19·60	36·11	45·87	25·90	36·73	—	—	—	—
S	18·64	30·85	45·67	24·64	36·84	28·49	48·40	10·36	38·18
(V)	—	32·71	—	—	36·84	27·41	—	9·00	44·02
W	19·70	38·93	46·47	27·18	34·66	26·26	51·72	—	—
X	16·57	—	—	34·19	—	—	—	—	—
Z	16·82	31·49	45·44	24·95	37·82	27·99	49·01	10·75	7·60

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The following table contains the means derived for the measured distances from the numbers of the preceding table; that part of the probable error of each mean which can be inferred from the discordances of the individual results; the longitudes (reckoned westward) resulting from summing up the means; the differences of these longitudes from those other determinations which have been referred to at the beginning of this paper.

TABLE III.

Section I.

Assumed Longitude of Starting Point: Devonport = 0h 16m 40s·53.

Link of Chain.	Mean of Chron. diff.	Weight.	Prob. Error (part.)	Resulting W. Longitudes.		Error.
	h m s				h m s	s
a	1 17 21·22	23	±0·29	Porto Praya	1 34 1·75	
b	1 0 4·19	21	0·38	Bahia	2 34 5·94	+0·52
1^1	0 18 31·98	$25\frac{1}{4}$	0·29			
1^2	30·73	$25\frac{5}{8}$	0·15			
1^3	31·47	25	0·30			
1 (mean)	0 18 31·40	$75\frac{7}{8}$	0·15	Rio de Janeiro	2 52 37·34	−0·61
2	0 52 17·17	25	0·45	Monte Video	3 44 54·51	−1·65

Section II.

Assumed Longitude of Starting Point: Monte Video = 3h 44m 56s·16 (*Error* − 0s·27).

3	0 38 39·92	$21\frac{7}{8}$	±0·67	Port Desire	4 23 36·08	
4	0 20 6·21	$19\frac{7}{8}$	0·70	Port Famine	4 43 42·29	−3·0*
5	0 11 49·36	$22\frac{5}{8}$	0·57	Ancud	4 55 31·65	
6	−0 9 1·69	$22\frac{1}{2}$	0·52	Valparaiso	4 46 29·96	−0·88

* Compared with most probable general result: −1s·79.

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Section III.

Assumed Longitude of Starting Point: Valparaiso = 4h 46m 29s·96 (*Error* −0s·88).

Link of Chain.	Mean of Chron. diff.	Weight.	Prob. Error (part.)	Resulting W. Longitudes.		Error.
	h m s		s		h m s	s
7	0 22 7·81	22¼	±0·58	Callao	5 8 37·77	+0·60*
8	0 49 32·31	23	0·32	Chatham Island	5 58 10·08	
9	0 3 39·49	24¾	0·23	Charles Island	6 1 49·57	
10	3 56 10·46	24¼	0·63	Otaheite	9 58 0·03	+4·13†
11	2 25 33·72	25¾	0·57	Paihia	12 23 33·75	
12	1 31 30·04	25¼	0·38	Sydney	13 55 3·79	−3·72‡
13	0 15 29·92	25¼	0·52	Hobart Town	14 10 33·71	
14	1 57 49·10	25	1·10	King George Sound	16 8 22·81	
15	1 24 6·43	24¾	0·54	Keeling Island	17 32 29·24	
16	2 37 33·54	24¼	0·53	Port Louis	20 10 2·78	
17	2 36 20·18	24½	0·58	Simon's Bay	22 46 22·96	+6·80
18	1 16 32·16	24½	0·74	St. Helena	0 22 55·12	+2·21§
19	0 34 45·29	24¾	0·12	Ascension	0 57 40·41	
20	1 36 25·45	25¼	0·36	Bahia	2 34 5·85	+0·43
v	−0 14 36·67	25¼	0·14	Pernambuco	2 19 29·18	+0·22
w	−0 45 27·86	22¼	0·33	Porto Praya	1 34 1·32	
x	0 14 48·80	21¾	0·23	Angra	1 48 50·12	
y	−1 32 10·01	20¼	0·28	Devonport	0 16 40·11	−0·42
z	−0 16 39·76	19¾	0·70	Greenwich	0 0 0·35	+0·35

* Compared with most probable general result: +0s·52.
† " " " " +2s·93.
‡ Compared with new telegraphic longitude: −4s·69.
§ Compared with most probable general result: +2s·92.

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The deviations of the individual reduced results from these means are contained in the following table :—

TABLE IV.

Differences of the Individual Reduced Measurements from the Mean.

Measured by Chron.	a	b	1¹	1²	1³	2	3	4	5	6
	s	s	s	s	s	s	s	s	s	s
A	+0·24	+ 0·27	+0·33	+1·09	− 0·42	− 1·21	− 2·29	− 1·62	+ 2·25	+ 1·65
B	+1·56	− 3·85	—	—	+11·52	− 7·57	—	−14·97	+ 9·72	+ 3·98
C	+3·29	+ 1·27	+0·25	+0·28	− 1·35	− 3·41	+ 2·46	+ 3·34	− 4·59	− 1·18
D	−1·13	(−20·60)	+1·09	+0 11	− 1·14	+ 2·15	+ 7·77	+ 1·21	− 3·51	− 5·49
E	−1·59	(−12·65)	—	+2·75	+ 2·31	− 2·92	+ 7·64	—	− 0 88	− 6·42
F	+1·49	− 0·43	+3·27	−1·23	+ 0·14	− 2·94	− 7·48	− 4·35	—	+12·49
G	−0·39	+ 1·24	+1·41	−0·32	− 1·38	+ 1·77	− 0·70	− 7·06	+ 4·27	+ 3·48
(H)	−6·48	—	−2·45	+0·14	—	− 8·43	+ 5·30	+ 4·93	− 4·77	− 5·37
(K)	−2·26	+ 9·11	−4·93	−0 72	− 6·17	+ 5·00	+ 8·26	− 9·53	+ 6·12	− 4·85
L	−0 24	− 2·72	−2·34	+0·47	− 0·43	+ 5·57	+ 1·30	− 0·27	+ 0·60	− 1·69
M	−1·95	− 5·77	+5·23	+0·72	—	—	− 4·41	+ 6·72	− 2·03	—
N	+1·64	− 1·72	−2·03	+0·70	+ 1·08	+ 0·98	− 3·29	—	+ 4·09	− 0·36
O	—	(−11·91)	+1·53	+0·26	− 0·58	− 0·36	—	—	—	—
P	−1·95	+ 0·85	+1·39	−0·22	+ 2·72	− 2·49	—	—	− 3·87	+ 3·05
R	−2·06	− 1·41	+0·79	−1·08	+ 1·21	+ 2·88	− 4·05	− 0·33	+ 2·95	+ 1·45
S	+3·90	−11·35	−0·94	—	+10·53	+ 0·01	+13·95	−13·02	− 0·66	—
(T)	—	−18·69	+1·05	+0·45	+ 2·19	− 2·83	+22·19	−23·21	—	—
(V)	−0·15	+ 5·88	−6·27	+0·70	− 0·90	—	—	—	+ 0·91	− 1·72
W	+1·57	+ 1·49	−2·52	−1·78	+ 1·68	− 1·04	+ 1·71	+ 5·53	− 3·72	− 3·50
X	+1·82	− 0·14	+0·32	−0·56	− 0·43	− 0·69	+ 8·78	+ 6·77	−10 96	− 4·58
(Y)	−7·44	− 8·18	+1·03	−3·21	− 7·66	+16·29	− 7·75	—	+ 5·60	+ 2·57
Z	+0·13	+ 0·42	−0·54	−0·47	− 0·60	+ 1·35	+ 0·97	− 0·08	− 1·21	+ 0·32

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Measured by Chron.	7	8	9	10	11	12	13	14	15	16
	s	s	s	s	s	s	s	s	s	s
A	−2·35	− 1·88	−0·30	−3·26	+ 1·20	+ 1·34	− 1·43	− 3·08	− 1·69	+ 2·84
B	−5·71	+ 0·94	−1·99	−0·27	+ 6·42	− 0·93	− 2·98	(−20·70)	+ 2·75	+ 4·56
C	−1·70	+ 0·87	−0·54	−5·29	− 2·85	+ 2·48	+ 3·91	+ 8·74	+ 1·20	+ 0·15
D	−2·62	+ 0·18	−0·48	−8·32	− 1·40	+ 0·24	+ 4·69	+12·79	+ 0·61	− 3·56
E	[−3·39]	—	—	—	—	—	—	—	—	—
F	[+8·79]	—	—	—	—	—	—	—	—	—
G	−5·16	—	+4·86	—	− 5·14	− 5·66	− 0·46	+ 1·15	+ 1·96	(−19·88)
(H)	—	—	—	+3·87	+ 2·18	− 5·96	—	+ 5·39	− 0·64	—
(K)	—	− 3·47	−3·16	—	+ 0·18	+12·15	− 1·09	−10·26	− 6·61	+11·59
L	—	—	−1·67	+4·10	−11·18	− 0·86	− 0·25	+ 2·39	+12·81	+ 6·13
N	—	+ 2·97	+1·88	−0·68	+ 3·00	+ 0·21	− 0·06	+ 5·75	− 0·68	− 3·13
O	+0·35	+ 0·54	−0·19	+2·74	+ 0·42	− 1·21	− 0·87	+ 1·56	+ 0·66	− 1·57
P	[+1·16]	—	—	—	—	—	—	—	—	—
R	+2·27	− 0·90	−0·26	+1·98	+ 0·91	− 0·52	− 4·25	− 7·93	+ 1·51	+ 2·66
S	+3·46	+ 0·18	−0·07	+0·65	− 5·09	+ 0·02	+ 2·04	+ 3·43	− 0·03	− 1·74
(V)	—	—	—	—	− 2·03	—	+ 9·80	—	—	− 5·48
W	+5·81	+ 3·70	−0·12	+0·44	+ 1·34	− 0·27	− 3·90	−11·46	− 4·68	− 2·22
X	−1·70	− 1·18	+0·16	+4·43	− 0·13	− 0·60	+ 1·62	+ 1·75	(−19·62)	− 2·65
Z	−0·40	− 0·58	−0·23	+2·45	+ 3·51	+ 0·51	+ 1·61	− 0·48	− 1·46	− 1·33

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Measured by Chron.	17	18	19	20	V	W	X	Y	Z
	s	s	s	s	s	s	s	s	s
A	+1·60	+3·35	−0·25	+0·36	−0·14	+2·04	−0·92	−1·12	+3·34
B	—	—	−0·47	−4·25	−0·48	+0·97	+2·12	—	—
C	−3·71	−2·65	−0·81	−0·42	+0·12	−0·62	−1·03	+0·69	+1·16
D	+5·53	−4·46	−0·96	+1·86	+1·06	−0·93	+1·04	−1·61	−4·05
G	+7·56	−4·81	−1·07	−2·93	−1·54	−3·11	+0·65	+0·91	−4·60
(H)	—	—	−1·08	−1·88	−0·03	−0·72	+1·73	−2·95	—
(K)	+0·88	+7·94	−0·90	−4·64	−1·86	+2·84	−2·10	—	—
L	−1·11	−5·74	+1·70	+1·45	+0·48	−5·88	−0·36	−0·67	−2·01
N	+1·18	−7·62	+0·11	+3·01	+0·59	+0·64	−1·89	−0·40	−3·96
O	+2·55	−0·66	−0·23	−2·19	+0·22	−1·48	+0·60	+2·39	−3·97
R	−0·58	+3·95	+0·58	+0·45	−0·06	—	—	—	—
S	−1·54	−1·31	+0·38	−0·81	−0·17	−0·63	−0·40	−0·35	+1·58
(V)	—	—	—	—	−0·17	−0·47	—	+1·01	−4·26
W	−0·48	+6·77	+1·18	+1·73	+2·01	+1·60	+2·92	—	—
X	−3·61	—	—	+8·74	—	—	—	—	—
Z	−3·36	−0·67	+0·15	−0·50	−1·15	−0·13	+0·21	−0·74	+2·16

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In Section I. the distribution of the outstanding errors might be still a little improved; changing, for example, the mean of the adopted values of $r_s - r_h$ by $+0^s.01$, the errors would become: Bahia, $+0^s.91$; Rio de Janeiro, $-0^s.15$; Monte Video, $-0^s.98$ (or $-1^s.25$, taking the correct mean of Fitz-Roy's and Fleuriais' reduction to the Cathedral). While the smallness of these outstanding absolute errors appears very satisfactory, the difference of the errors for Bahia and Rio, which cannot be diminished by any possible change in the reduction, is somewhat striking, proving the mean of three very nearly agreeing measurements of this meridian distance to be in error by more than a whole second.

In Section II. the reliableness of the reduction can only be tested by the determination of Port Famine. Bearing in mind the limited accuracy of the connection of my new longitude for Punta Arenas with that point—details of which will be found hereafter—the harmony of both results can be regarded as quite satisfactory. On the contrary, the extremely near agreement of all other chronometrical measurements made in this region, by the "Beagle" in her first cruise, during the course of the second expedition by the tender "Adventure," and by Captain Foster in the "Chanticleer," with those results which were adopted by Fitz-Roy,* and the large and regular discordance, therefore, of all of them from my results obtained for Section II., might look astonishing; but it cannot be used as an argument against the correctness of these latter results. All these measurements are entitled only to a small weight; † and the numbers derived from them, together with those given by Fitz-Roy, cannot stand against the fact of their all giving, equally as these latter, the length of the line Monte Video—Valparaiso about 16^s too long. ‡

* *Narrative*, vol. ii. App. pp. 348, 349.

† The "Beagle" in her first voyage carried only three chronometers, the "Adventure" (tender 1833–4) probably two. The "Chanticleer" was equipped with a great number of chronometers (17), but their rates were generally irregular, apparently from their being improperly mounted on board.

‡ I will not omit to mention, that another trial to control the reduction of Section II. gave a result at first very perplexing. The longitude of S. Cruz, French Observatory, Transit of *Venus* 1882, according to M. Fleuriais' statements in *Rapports préliminaires*, pp. 61 foll., results:—

	h	m	s
From lunar transits	4	34	10·43
From occultations			12·72
By chronometers (French) from Monte Video ...			12·02
By chronometers (Argentine) from Punta Arenas			12·12

The first of these results, notwithstanding the excellent agreement between the three observers, appears to be some seconds too small, like M. Fleuriais' determinations made 1867–70; the mean of the three other results as they are provisionally given $= 4^h\ 34^m\ 12^s.29$ certainly can be considered to be a very near approximation to the true longitude. Now the French Expedition has connected the Observatory, among other points, with the American pyramid "at a small distance from Keel Point" and with Mount Entrance, the positions

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As to Section III., any considerable improvement of the results arrived at in the preceding investigation could no more be hoped for, as the values of $r_s - r_h$, resulting from the line Valparaiso—Greenwich, were found not differing materially from those applied to the whole chain in the first reduction.

If the correction of $+0^s.88$, which the longitude of Valparaiso adopted in the reduction of Section III. appears to require, be duly distributed over the line, the outstanding errors for the places affording other determinations become:—

	s		s		s
Callao ...	+1·43	Simon's Bay ...	+7·09	Pernambuco ...	+0·38
Otaheite ...	+4·82	St. Helena ...	+2·44	Devonport ...	−0·39
Sydney ...	−3·15	Bahia ...	+0·61	Greenwich ...	+0·35

The error does not vanish for Greenwich, because, as may be seen from the table, for some incomplete chains the adopted values of $r_s - r_h$ have been used beyond the ends of the lines from which originally they had been deduced. I have left this error standing as it came out, because at Devonport the error lies in the opposite direction; and, moreover, the measurement

of these points resulting from the above value of the longitude and the latitude given by M. Fleuriais for his Observatory $= -49° 59' 21''.5$:—

American pyramid	...	68°	24′	43″	−50°	7′	11″
Mount Entrance ...	...	68	21	31	−50	8	52

Fitz-Roy places in his Table of Positions:

Keel Point in	...	68°	23′	30″	−50°	6′	45″
Mount Entrance in	...	68	20	0	−50	8	50

His longitudes, therefore, in this region require to be *increased by* 5^s *or* 6^s.

On the contrary, I have derived in the reduction of Section II. the corrections to be applied to his fundamental meridians of Port Desire and Port Famine as $= -4''.72$ and $-9''.21$ respectively, and the mouth of the River S. Cruz being situated nearly midway between these two places, it was to be expected that his longitudes in that region would require a *diminution* equal nearly to the mean of these two corrections, *say* -7^s.

From my new computation, therefore, seemed to arise a difference reaching to the most perplexing amount of 12^s or 13^s; but on closer inspection it became evident that the longitudes of that part of the Patagonian coast depend upon a chronometrical insertion between Port Desire and Port Famine made in the course of the first expedition of the "Adventure" and "Beagle," which appears to have been a failure from the fact of falling short by 9^s of the trigonometrical transport of longitude effected on the same occasion.

This example shows very plainly how precarious it is, as has been done of late, to shift in tracts the shores of South America as laid down by Fitz-Roy, according to new determinations of single points lying within or near the tract. The measurements to the south of a line drawn from the mouth of the River Plate to the Peninsula of Tres Montes are so entangled with each other, that a certain correction of the secondary points from new determinations of the primary points can only be effected by tracing the determination of each place to its very foundation, generally, therefore, only by recurring to the originals of the measurements. Otherwise the changes will remain for this region more or less arbitrary, and more likely to produce confusion than correction.

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of the last link of the chain, Devonport—Greenwich, appears subject to an uncertainty considerably above the average.*

The errors in Section III. would, if considered singly, scarcely appear very large, except at the Cape of Good Hope; but, from the rapid variation of the error in the comparatively short distances from Otaheite to Sydney, and from the Cape to Bahia, and from the still larger variation in the line, very long indeed, from Sydney to the Cape, the results cannot but appear very unsatisfactory.

These variations are far greater than could be expected, if the discordances of the chronometers from each other would lead to an approximately true idea of the accuracy of the transports of local time, as is seen by comparing the parts of the probable errors given in Table III. That the chronometers, besides the merely accidental disturbances which alone are shown by these discordances, have been influenced by causes of

* The connections with Falmouth and Portsmouth, included in meas. y and z, do not afford reasons to change the above values.

The difference Falmouth—Angra reduced for 8 days' duration of run becomes from:—

	h m s	s		h m s	s
A	−1 28 39·82,	Diff. +0·26	N	−1 28 41·37,	Diff. −1·29
C	40·56	−0·48	O	38·91	+1·17
D	42·50	−2·42	S	41·55	−1·47
G	38·84	+1·24	(V)	39·62	+0·46
(H)	42·91	−2·83	Z	39·19	+0·89
L	40·08	0·00			

the mean being = −1h 28m 40s·08 (weight, 20¼). Therefore, from Section III. of the chain the longitude of Falmouth, Pendennis Castle, would be 0h 20m 10s·04, and the error, compared with the longitude derived from the correction of Mudge's determination, as stated above, −0s·89. Dr. Tiarks found by 15 chronometers, 1822, on the outward voyage from Greenwich to Madeira, 20m 11s·49, and on the return voyage 20m 11s·13, the mean being 20m 11s·31, and affording a value apparently very trustworthy, as these voyages have given the longitude of Madeira remarkably correct. Hence the error would be = −1s·27; and comparing with the longitude 20m 10s·81 as resulting from Dr. Tiarks' connection with Dover, effected in 1823 by repeated voyages with 26 chronometers, it would become = −0s·77.

The continuation of the chain from Devonport to Portsmouth is possible with only nine chronometers. Assuming the run to have been 2 days, from the numbers given in the foot-note to Table I. the reduced values of the difference, Portsmouth—Devonport, will follow:—

	m s	s		m s	s
A	−12 12·85,	Diff. +1·77	N	−12 15·79,	Diff. −1·17
C	15·23	−0·61	O	14·30	+0·32
D	16·15	−1·53	S	14·83	−0·21
G	16·17	−1·55	Z	14·75	−0·13
L	16·04	−1·42			

mean = −12m 14s·62 (weight, 19¼), making the longitude of Portsmouth from the chain, Section III., 0h 4m 25s·49; error, compared with Trig. Survey corr. = +0s·69 (compared with N.A. = +1s·59). The errors at Falmouth and Portsmouth, therefore, are at all events, as at Devonport and Greenwich, of nearly the same amount with opposite signs.

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errors affecting the majority of them, in the same direction, under the same circumstances, and appearing only from a comparison with known longitudes, is shown very plainly by an inspection of those longitudes which would result from each one of the nine best chronometers, forcing them into agreement at the ends of the line with the values $4^h\ 46^m\ 29^s{\cdot}96$ and 0, for the four intermediate points, Otaheite,* Sydney, Simon's Bay, and Bahia. The errors would become at these places from the different chronometers :—

A	C	D	N	O	R	S	W	Z
s	s	s	s	s	s	s	s	s
−4·3	− 3·1	− 7·7	+ 6·4	+ 7·0	+6·6	+7·7	+13·4	+4·8
−9·0	−10·7	−16·1	+ 2·4	− 1·1	−0·3	−4·6	+ 7·2	+1·6
−0·2	+10·1	+14·5	+16·0	+11·8	+1·7	+8·1	− 5·0	+7·1
−3·1	− 0·2	+ 4·5	+ 5·1	+ 2·3	+0·3	+0·0	− 1·7	−0·3

The change of the error therefore is :—

Between	A	C	D	N	O	R	S	W	Z
	s	s	s	s	s	s	s	s	s
Valparaiso & Otaheite	−4·3	− 3·1	− 7·7	+ 6·4†	+ 7·0	+6·6	+ 7·7	+13·4	+4·8
Otaheite & Sydney	−4·7	− 7·6	− 8·4	− 4·0	− 8·1	−6·9	−12·3	− 6·2	−3·2
Sydney & Cape G. H.	+8·8	+20·8	+30·6	+13·6	+12·9	+2·0	+12·7	−12·2	+5·5
Cape G. H. & Bahia	−2·9	−10·3	−10·0	−10·9	− 9·5	−1·4	− 8·1	+ 3·3	−7·4
Bahia & Greenwich	+3·1	+ 0·2	− 4·5	− 5·1	− 2·3		− 0·0		+0·3

The change is greatest in the three middle divisions; and it ought to be remarked that in one of them all the nine chronometers, in the two others eight out of the nine, have changed in the same direction.

Errors in the determination of local time can have contributed but little to these common deviations. The possible influence of such errors upon the measurement of the single differences cannot be made out exactly, because the dates of the four determina-

* In drawing up the above tables, the longitude of Point Venus was assumed, slightly different from the most probable value to be given at the end of this paper, $9^h\ 57^m\ 56^s{\cdot}5$. Allowing for the difference, and increasing the longitude of Valparaiso by the apparently necessary amount, the variations would be smoothed down to some extent between Valparaiso and Sydney—and a little more so by substituting the new telegraphic value of the longitude of the latter place—but, the general character of the second table remaining sensibly the same, I have thought it unnecessary to substitute the later values.

† Between Callao and Otaheite.

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tions used in each single measurement are not known, but the following numbers cannot be much in error:—

Corresponding part of probable error of the connection of	Valparaiso and Otaheite ...	$= 3{\cdot}7\,\epsilon''$
„ „	Otaheite and Sydney ...	$6{\cdot}3\,\epsilon''$
„ „	Sydney and Simon's Bay ...	$8{\cdot}1\,\epsilon''$
„ „	Simon's Bay and Bahia ...	$5{\cdot}2\,\epsilon''$
„ „	Bahia and Greenwich ...	$9{\cdot}1\,\epsilon''$

ϵ'' denoting the probable error of a complete determination of local time. As ϵ'' will not have exceeded some few tenths of a second, this source of error cannot have produced, except in the last division, any appreciable increase of the actual total error, which becomes evident from the comparison of the longitudes.

The errors of compensation, according to all probability, will have contributed to a certain extent towards the large outstanding differences. These errors, indeed, may be assumed to have been generally similar, especially in the good chronometers, while the variations of temperature happening within the limits of the separate measurements appear from the "Abstract of the Meteorological Journal" to have reached sometimes such amounts as, without assuming extraordinary values of the errors of compensation, might be considered to have produced an appreciable difference of the rate during the run from the mean of the rates determined before and after the run. I have made some trials to arrive, by the aid of the temperatures given in the Abstract, at such determinations of the temperatures of most perfect compensation and of the coefficients of the square, by which the changes of the errors in the five divisions might be reduced as much as possible, but upon the whole without success: on one side because the data of the Abstract proved too inadequate to determine the true temperature of the chronometers; on the other doubtless because errors of another nature have contributed too much to the discordances. From a comparison of the following numbers again the variations of temperature will not appear to have been the principal cause of them:—

Mean.	Interval.	Obs.	$\epsilon'\epsilon'$ for 20 d.	$\tau_h'' - \tau_h'$	$\tau_s - \tau_h'$	$\tau_s - \tau_h''$	Mean of Temp.
	d			°	°	°	°
a	23	4·39	3·82	+24:	+12:	−12	64
b	26	6·78	5·22	+ 6	+ 4	− 2	79
1^1	22	4·59	4·17	− 6	0	+ 6	80
1^2	6	1·27	4·23	+ 4	0	− 4	77
1^3	14	5·08	7·26	− 6	− 4	+ 2	77
2	2	10·94	9·12	−16	− 8	+ 8	66

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Meas.	Interval.	Obs. $\varepsilon\varepsilon'$	for 20 d.	$\tau_h''-\tau_h'$	$\tau_s-\tau_h'$	$\tau_s-\tau_h''$	Mean of Temp.
	d			°	°	°	c
3	19	21·81	22·96	− 8	− 7	+ 1	64
4	16	21·16	26·45	− 7	− 2	+ 5	58
5	27	16·10	11·93	+ 7	+ 6	− 1	47
6	12	13·26	22·10	+ 6	+ 5	− 1	54
7	25	16·20	12·96	+ 7	+ 5	− 2	62
8	12	5·27	8·78	+ 7	+ 4	− 3	69
9	4	2·79	13·95	--	—		72
10	31	21·48	13·86	+ 8	+ 5	− 3	76
11	28	18·61	13·29	− 11	− 5	+ 6	75
12	19	8·12	8·55	+ 4	+ 2	− 2	71
13	11	15·05	27·36	− 6	− 6	0	69
14	20	66·87	66·87	+ 1	− 3	− 4	66
15	20	15·76	15·76	+ 14	+ 6	− 8	75
16	21	14·95	14·24	− 3	− 1	+ 2	81
17	25	17·16	13·73	- 15	− 6	+ 9	72
18	21	29·31	27·91	+ 7	+ 3	− 4	68
19	7	0·79	2·26	+ 5	+ 2	− 3	73
20	10	7·13	14·26	+ 2	+ 1	− 1	77
v	7	1·12	3·19	0	0	0	78
w	14	5·28	7·54	+ 3	+ 3	0	80
x	15	2·55	3·40	− 6	− 3	+ 3	78
y	14	3·44	4·42	− 16	− 9	+ 7	67
z	11	21·18	38·50	− 6 :	+ 1	+ 7 :	57

Here "$\varepsilon'\varepsilon'$ obs." is the square of the mean error for weight 1, resulting, for each single difference of meridians, from the deviations of the individual results from their mean, and this value has been reduced in the next column to an interval of twenty days, assuming $\varepsilon'\varepsilon'$ to vary proportionally to the length of the intervals. The last four columns contain the differences between the mean temperatures—in the meaning explained before—in which the chronometers were rated (τ_h' and τ_h'') and transported (τ_s), and the means of these three temperatures.

Two values of $\varepsilon'\varepsilon'$ reduced to the unit of time appear most extraordinarily large. They belong to those two measurements in which τ_s has not been, as in all the other measurements, included between the limits τ_h' and τ_h'', but was outside of them. This circumstance indeed would seem to point to errors of compensation being the cause, and really such errors may be the principal cause of the unusual want of harmony between the individual results in the last measurement, as possibly τ_h''

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has in this case been in reality much lower than assumed in the table.* The abnormal discrepancy of the chronometers in the passage from Hobart Town to King George Sound (meas. 14) must have been produced by other, unknown, circumstances; and besides these two instances there appears nowhere from the table any indication of a connection between the amount of the discordances, and the variations, or the absolute amount of temperature.

The effects of temperature, therefore, which to a certain extent may have contributed towards the outstanding errors, appear to be concealed by larger errors of a different kind. Indeed the assumption upon which the computation of Section III. has been founded, though indispensable, is necessarily erroneous, because the chronometers (which moreover had not been cleaned for four years) would not in reality maintain a uniform performance through so long a period as seventeen months, and under the varying circumstances of the different runs. It will chiefly be the errors of this assumption, viz., the actual variations of $r_s - r_h$, which show themselves in the outstanding errors of the longitudes; and as to the remarkable general agreement of these errors, and, correspondingly, of the variations of $r_s - r_h$ for the different chronometers, it might seem *a priori* quite probable, that the amount and character of the ship's motion under the influence of wind and sea would have influenced the great majority of the chronometers in the same direction.

Possibly also an influence of terrestrial magnetism has contributed to the irregularities occurring within the chain.† It is not possible to examine this closely, because no statements are given as to the ship's position during the rating of the chronometers.

For the longitudes independent determinations cannot be nearer approximated from the published data, unfortunately incomplete in so many respects, relating to the "Beagle's" measurements, than has been done in Table III. Therefore the present investigation might be looked upon to a certain degree less as a matter of practical value than of personal interest in an undertaking which, in my opinion, has been most admirably managed both in its nautical and scientific aspects; but I consider such results entitled to present value, as will become available from this investigation, for determining longitudes not yet settled in a more unobjectionable way, by interpolating the error of the chain between the nearest well-known places. The longitudes resulting from the "Beagle's" measurements are now entirely superseded for Pernambuco, Bahia, Rio de Janeiro, Monte Video,

* See *Abstract of Met. Obs.* p. 62.

† A chronometer, which recently passed through my hands, of very superior quality in other respects, had its daily rate changing to an amount of nearly 3ˢ, according to the magnetic azimuth in which a certain diameter was placed.

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Valparaiso, Sydney, and the Cape of Good Hope. It will be the same with Callao, Hobart Town, and Port Louis (Mauritius), when the results of the determinations of the longitudes of these places, recently made, will be known. The "Beagle's" longitudes are entitled still to contribute, with some weight, to the determination of Port Famine, and of one place of considerable astronomical interest, Point Venus; for the Azores group, for Porto Praya, the two astronomically interesting islands, St. Helena and Ascension, further for Port Desire, Ancud, the Galapagos group, Paihia, King George Sound, and Keeling Islands, they still afford the most accurate results.

The following is a summary of those of the "Beagle's" results which may be considered to have practical value in the present day:—

	h	m	s	
Porto Praya, Island of St. Jago.				
Longitude found in Section I.	1	34	1·75	W.
Correction interpolated between Devonport and Bahia			−0·24	
Longitude corrected	1	34	1·51	W.
Longitude found in Section III.	1	34	1·32	W.
Correction interpolated between Pernambuco and Devonport			−0·01	
Longitude corrected	1	34	1·31	W.
Mean of the two determinations	= 1	34	1·41	W.
Angra, Terceira.				
Longitude from Section III.	1	48	50·12	W.
Correction interpolated from Porto Praya (mean as before) and Devonport			+0·26	
Longitude corrected	1	48	50·38	W.
St. Helena.				
Longitude from Section III.	0	22	55·12	W.
Correction interpolated between Simon's Bay and Bahia			−3·28	
Longitude corrected	0	22	51·84	W.
Johnson's longitude, as corrected above, is ...	0	22	52·91	W.
The most probable result will at present be, allowing weights as 2 : 1	0	22	52·20	W.
Ascension.				
Longitude from Section III.	0	57	40·41	W.
Correction interpolated as for St. Helena ...			−2·11	
Longitude corrected	0	57	38·30	W.

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Port Desire.

	h m s
Longitude from Section II.	4 23 36·08 W.
Correction interpolated from Monte Video (+0"·27) and Port Famine (see below) ...	+1·10
Longitude corrected	4 23 37·18 W.

S. Carlos de Ancud, Chiloë.

Longitude from Section II.	4 55 31·65 W.
Correction interpolated from Port Famine and Valparaiso (+0"·88)	+1·16
Longitude corrected	4 55 32·81 W.

Chatham Island, Galapagos.

Longitude from Section III.	5 58 10·08 W.
Correction interpolated from Callao (see below) and Point Venus (see below)	−1·13
Longitude corrected	5 58 8·95 W.

Charles Island, Galapagos.

Longitude from Section III.	6 1 49·57 W.
Correction interpolated as before	−1·34
Longitude corrected	6 1 48·23 W.

Paihia, Bay of Islands, New Zealand.

Longitude from Section III.	11 36 26·25 E.
Correction interpolated from Point Venus and Sydney	−1·03
Longitude corrected	11 36 25·22 E.*

King George Sound.

Longitude from Section III.	7 51 37·19 E.
Correction interpolated from Sydney and Simon's Bay	−0·35
Longitude corrected	7 51 36·84 E.*

Keeling Islands.

Longitude from Section III.	6 27 30·76 E.
Correction interpolated as before	+1·82
Longitude corrected	6 27 32·58 E.*

The last two results are open to improvement, as soon as the correction can be derived from the new determinations of Hobart

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Town and Port Louis. From the "Beagle's" measurements the longitudes of these places would be, interpolating the correction to be applied to the values derived in Section III. from Sydney and Simon's Bay:—

	h m s	s	h m s
Hobart Town ...	9 49 26·29	−2·52	=9 49 23·77 E.*
Port Louis ...	3 49 57·22	+4·09	=3 50 1·31 E.*

Finally we have, combining the "Beagle's" results with values already used in the preceding investigation:—

Port Famine.

	h m s
Longitude from Section II., interpolating between corrections +0·27 for Monte Video and +0·88 for Valparaiso (+0·56)	4 43 42·85 W.
By connections with Punta Arenas (see note)	4 43 45·3 W.
Probable mean	4 43 44·08 W.

Callao.

	h m s
Longitude from Section III.	5 8 37·77 W.
Correction interpolated from Valparaiso (+0·88) and Point Venus (−2·93 according to following determination) ...	−0·44
Longitude corrected	5 8 37·33 W.
From telegraphic connection with Pisco ...	5 8 37·17
Probable general result, mean	5 8 37·25 W.

Point Venus, Otaheite.

	h m s
Longitude from Section III., allowing for the correction to Valparaiso	9 58 0·72 W.
Mean of three determinations given above	9 57 55·9
Probable general result, allowing weights as 1 : 3	9 57 57·10 W.

In his summary of the results for the great chain, vol. ii. App. pp. 347–350, Fitz-Roy has given determinations, besides those for the places the longitudes of which have been anew deduced in the preceding investigation, for Fernando Noronha, Port Louis (Falkland Islands) and Cape Horn, but without giving any details of these determinations. The last two places can only be supposed to have been determined when the "Beagle" sailed from Port Famine to Berkeley Sound in February and March 1834, therefore belonging to Section II., whence it would

* Substituting the new determination for Sydney, the corrections would be interpolated as −1ˢ·61, −1ˢ·02, +1ˢ·35, −3ˢ·39, +3ˢ·82, and the resulting longitudes of the places depending upon Sydney would become: Paihia 11ʰ 36ᵐ 24ˢ·44 E., King George Sound 7ʰ 51ᵐ 36ˢ·17 E., Keeling Islands 6ʰ 27ᵐ 32ˢ·11 E., Hobart Town 9ʰ 49ᵐ 22ˢ·90 E,, Port Louis 3ʰ 50ᵐ 1ˢ·04 E.

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be inferred, that to both places considerably too great longitudes have been assigned.

Fernando Noronha was determined in the passage from Porto Praya to Bahia, 1832. I may add some few words to establish the most probable result of this determination and of some others, which are available to fix the longitude of this island, which, both by position and shape, is so admirably suited to rate chronometers on board ships traversing the equatorial part of the Atlantic Ocean.

The means, alone given by Fitz-Roy, for the distances of Fernando Noronha from the two including meridians (ends of arc **b**) most probably depend exclusively upon the same ten chronometers, from which, as from the ten best agreeing, he derived his final result for arc **b**; and further it is probable that the numbers originally given by their mean have been:—

Meridian distance from Porto Praya to Fernando Noronha (observations, 1832 Feb. 5 and Feb. 20, run 12 days) $35^m\ 39^s{\cdot}6$.

Meridian distance from Fernando Noronha to Bahia (observations, 1832 Feb. 20 and March 2, run 7 days) $24^m\ 23^s{\cdot}4$.

These values ought to be reduced by the mean of the values of $r_s - r_h$ found in Section I. for the ten chronometers $= -0^s{\cdot}0094$, whence the longitude becomes

	h	m	s	m	s	h	m	s
From Porto Praya ...	1	34	1·40	+35	39·49	=2	9	40·89
From Bahia	2	34	5·42	−24	23·33	=2	9	42·09

the mean, with weights as 11 : 15, being $2^h\ 9^m\ 41^s{\cdot}58$. The observations are said to have been made as nearly as possible at the same spot as those of Foster,* the reduction to the pyramid therefore is to be assumed $= +2^s{\cdot}22$, and the longitude of this point becomes $2^h\ 9^m\ 43^s{\cdot}80$.

Two more determinations can be considered as equally trustworthy: one made by Lee (cruise of the "Dolphin" 1851–2), $9^m\ 47^s{\cdot}27$ E. of Pernambuco, Fort Picão,† making the longitude $= 2^h\ 9^m\ 40^s{\cdot}50$; the other by Mouchez, $42^m\ 56^s{\cdot}53$ E. of Rio de Janeiro, Fort Villegagnon,‡ making the longitude $= 2^h\ 9^m\ 41^s{\cdot}42$. The mean of the three determinations becomes:—

	h	m	s
Fernando Noronha, Pyramid,	2	9	41·91 W.

Note.

The amount of uncertainty in the connection of Port Famine with the German Observatory at Punta Arenas will appear from the following data. When the "Beagle" surveyed the Straits, the settlement at Punta Arenas did not yet exist, being established at this point now only thirty years ago. The nearest points laid

* *Narrative*, vol. ii. p. 59. *Voyage of H.M. Sloop "Chanticleer,"* vol. ii. p. 238.

† *Conn. d. T.* 1858, Add. p. 60; 1873, Add. p. 18.

‡ *Ib.* 1870, Add. p. 86.

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down in the Survey have been "Sandy Point extremity," the end of the broad flat point which has given its name to the settlement, lying 2½ nautical miles to the N.E.—while Port Famine is nearly 30 miles distant to the south of the settlement —and Cape Negro, distant 14 miles towards the north. Now the positions assigned to these two points in the tables given as results of the expeditions of the "Adventure" and "Beagle" do not agree, King making them respectively 4′ 30″ and 8′ 31″ to the E. of Port Famine,* Fitz-Roy 5′ 45″ and 8′ 45″ E.† As the "Table of Positions" given by Fitz-Roy would appear to contain the combined results of both expeditions, we might assume the latter values to be more accurate, and the first determination of Sandy Point extremity to be affected by some error. But in 1867 M. Fleuriais connected both these points by a triangulation with his Observatory—situated almost exactly on the same spot as that occupied by the German Observatory 1882—and found the difference of their longitudes almost exactly agreeing with King, placing "Sandy Point extremity" 2′ 49″·8, Cape Negro 6′ 47″ to the E. of his Observatory.‡ Applying the difference: German Observatory W. of French transit $0^{s}{\cdot}01$, Port Famine therefore would be found W. of German Observatory, Punta Arenas:—

	s
From Sandy Point extremity, by Fleuriais and King ...	6·67
„ „ „ Fitz-Roy ...	11·67
From Cape Negro, by Fleuriais and King	6·94
„ „ Fitz-Roy	7·87

The difference again existing between Fitz-Roy and Fleuriais, or part of it, cannot be explained by assuming the flat to have considerably changed in the interval, as from the locality the change would probably happen in the opposite direction; and I should be inclined to explain the differences between all the three determinations by the indistinctness of the point to be observed in the case of "Sandy Point extremity," the different observers being likely to have pointed, from a distance, at largely different spots of the broad flat "Point."§ On the contrary, Cape Negro is a good mark, and the possible doubt as to the identity of the objects pointed at would seem to be confined within the limits of some few seconds of arc on the earth's surface.‖ I should not hesitate, therefore, if these

* *Narrative*, vol. i. p. 481.

† *Ib.*, vol. ii. App. pp. 72, 74, 76.

‡ *Conn. d. T.* 1871, Add. p. 21.

§ Fleuriais' measurements differ also in the latitude of Sandy Point extremity considerably—more than 1′—from the two English determinations, which agree in this coordinate, while in the latitude of Cape Negro all the three determinations agree to a few seconds.

‖ The limits would be so narrow, if M. Fleuriais by "Cape Negro" means at all the same object as King and Fitz-Roy, viz. the N.E. point of Laredo Bay. The *highest* point, however, of the ridge forming the N. shore of Laredo Bay is situated considerably (nearly 1′) towards W.

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measurements alone were available, to take for each point the mean of King's and Fitz-Roy's differences, allowing double weight to the reduction from the more distant but better point.

Another determination, however, can be made by means of one of the very best landmarks in the Straits, Mount Tarn. From some angles taken in the vicinity of the German Observatory, the true bearing of Mount Tarn—peak at north end—from the transit pier, = S. 6° 45′ 25″ W. Mount Tarn, from the fact of being only eight nautical miles distant from Port Famine, and allowing of a very distinct pointing, would appear to be very accurately placed relatively to this latter point by the differences resulting from the table of positions: long. 4′ 25″ W., lat. 6′ 51″ S. The absolute value of the latitude of Port Famine appearing very accurate too, the resulting latitude of Mount Tarn = −53° 45′ 6″ compared with the latitude of the German Observatory = −53° 9′ 39″·35 can be safely used to derive from the observed azimuth the difference of longitude, which becomes 7′ 6″·14, whence Port Famine would be 2′ 41″·14 = 10ˢ·74 W. of Punta Arenas, sufficiently agreeing with the reduction from Fitz-Roy's determination of Sandy Point extremity, but widely inconsistent with King's, and with the result from the apparently better of the two northern points.

In this confused state of things, I can at present only take a simple mean from the determinations by the three different points, using for the two northern the means from King and Fitz-Roy. Then Port Famine becomes W. of Punta Arenas from Sandy Point extremity, 9ˢ·17; from Cape Negro, 7ˢ·40; from Mount Tarn, 10ˢ·74; mean = 9ˢ·10; whence the longitude of Port Famine is found, as made use of in the comparisons of the preceding investigation, = 4ʰ 43ᵐ 45ˢ·3 from a result for the longitude of the German Observatory, which is not yet the definitive, but cannot differ from the definitive by more than a few tenths of a second.

Berlin: March 1884.

Errata.

Page 228, third line from top, *for* $s = a + \left(1 + \frac{n - n_0}{n^\circ}\right) . t + (n - n_0) . t$,

read $s = a + 24^h \left(1 + \frac{n - n_0}{n^\circ}\right) . t + (n - n_0) . t$.

„ 231, seventh line from top, *for* $= t' \left(1 + \frac{\delta n}{360^\circ + n}\right)$,

read $t = t' \left(1 + \frac{\delta n}{360^\circ + n}\right)$.

„ 232, second line from top, *for* $s = a + 24^h\, t_0 + \frac{n + \delta n}{15} . t$,

read $s = a + 24^h\, t_0 + \frac{n + \delta n}{15} . t'$.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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EDWIN DUNKIN, F.R.S., President, in the Chair.

Charles Michie Smith, B.Sc., F.R.S.E., Professor of Physical Science, Madras Christian College; and

Arthur Stanley Williams, Walsingham House, West Brighton;

were balloted for and duly elected Fellows of the Society.

The Observations of the Moon made at the Radcliffe Observatory during the Year 1883, and a Comparison of the Results with the Tabular Places from Hansen's Lunar Tables. By E. J. Stone, M.A., F.R.S.

The present paper contains the Right Ascensions and North Polar Distances of the Moon as deduced from the observations made at the Radcliffe Observatory during the year 1883. I have compared these results with those deduced from Hansen's Lunar Tables on two suppositions:

1. That the mean times, found in the usual way from the sidereal times at mean noon given in the *Nautical Almanac*, were not changed in 1864;

2. That the mean times were changed in 1864 in accordance with the views which I have explained in Papers already communicated to the Society.

The corrections which I have applied to the mean times are computed from the expression

$$+0^{s}.55+24^{h}\times\frac{0.060216}{1296027.618184}\times\text{days from 1864 Jan. 1,}$$

and are added to the adopted sidereal times at each mean noon since 1864. Here $+0^{s}.55$ is a constant, but the second term is a variable one, and soon becomes important. The necessity for such a correction, after the adopted value of the Sun's mean

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motion in longitude was changed in the expression of the sidereal time in terms of the mean solar time from n to $n+\delta n$,

$$\frac{\delta n}{n} = \frac{0{\cdot}060216}{1296027{\cdot}618184},$$

follows from exactly the same reasoning as that which compels us to replace the sidereal time

$$= s = a + (24^h + n)t,$$

when t is expressed in days by

$$s = a + 24^h\left(1 + \frac{\delta n}{n}\right)t' + (n + \delta n)t',$$

or

$$s = a + 24^h \times 365{\cdot}25t' + 365{\cdot}25\, nt',$$

when we adopt instead of a day a Julian year of 365·25 such days as our unit. In this case

$$\frac{\delta n}{n} = 364{\cdot}25;$$

but if we were to omit the term

$$24^h . \frac{\delta n}{n} . t',$$

and to write

$$s = a + 24^h t' + 365{\cdot}25\, nt',$$

and then to assume that at the r^{th} *day* from epoch we could take $t' = r$, and thus find sidereal time at r^{th} *mean noon* $= 365{\cdot}25\, nr$, we should have made identically the same error in theory which we have made since 1864. The only difference is that when

$$\frac{\delta n}{n} = 364{\cdot}25$$

the errors which arise from such mistakes would be sufficiently large to prevent them being made in practice, and such errors have, therefore, never been made in practical astronomy; but when

$$\frac{\delta n}{n} = \frac{0{\cdot}060216}{1296027{\cdot}618184},$$

the errors accumulate so slowly that it is only after a lapse of some years that their effects become sufficiently sensible to be separated from personal errors and the defects of Lunar theory. But, if the omission of the term

$$24^h . \frac{\delta n}{n} . t'$$

is absurd when

$$\frac{\delta n}{n}$$

is large, it cannot be correct to omit this term when

$$\frac{\delta n}{n}$$

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is small. As my theoretical views have been disputed, I have shown that this correction does, as a matter of fact, remove the discordances between Hansen's Lunar Tables and observations over the whole period of twenty years which have elapsed since the change in the units of time took place. The following are the facts bearing on this question :—

(1) Hansen's Lunar Tables represented the motions of the Moon with great accuracy for many years before 1863. The mean yearly error, 1847–1864, was always small, and generally about $-1''.5$.

(2) In 1864 we changed the scale upon which our mean times are measured, or the ratio of the sidereal day to the tabular day. This is now admitted.

(3) I have given mathematical proofs which no one, at all events at present, has shown to be in error, that this change of units requires the correction

$$24^h . \frac{\delta n}{n} . t$$

to be applied to our present sidereal times at mean noon, in order to obtain the sidereal time at Bessel's old mean noon to which our tabular quantities refer. This correction is equivalent to a change of the adopted unit of time in the proportion of

$$1 : \left(1 + \frac{\delta n}{n}\right).$$

(4) When this correction is applied to our mean times, the discordances between Hansen's Lunar Tables and observations for the twenty years 1864–1883 are very little greater than they were before 1863.

The statements which have been made that the times are thus over-corrected do not appear to be warranted by the facts. The mean error in longitude was negative for years before 1864, and should, therefore, be negative now, if no change had been made and Hansen's Tables were perfect. The changes in the errors of Hansen's Tables are very small, as shown by the results. The mean of the four greatest negative errors in the comparison of Hansen's Tables in longitude with the Greenwich observations was $-9''.08$ in 1862, and $-6''.95$ in 1863; whilst the mean of the four greatest positive errors was respectively $+1''.51$ in 1862, and $+3''.52$ in 1863. The Radcliffe observations in 1882 and 1883 show corresponding mean errors of $-7''.17$ and $-6''.60$, and $+3''.93$ and $+3''.50$ respectively. There is therefore no over-correction. The error in time increases from $0^s.55$ at the rate of about $1^s.46$ per annum, but Hansen's Tables correctly represent the motion over the twenty years. This cannot be the result of an accident: the quantities dealt with are too large.

As my paper which gave the separate results of a comparison between Hansen's Lunar Tables and the Greenwich Observations 1864–1881, when the mean times were corrected in accordance with my theoretical views, has not been printed, I have appended to this paper the mean annual results.

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TABLE I.

Radcliffe Observations of the Moon, 1883.

Corr. to be subtracted from M.T. computed directly from Sid. Time at Mean Moon from N.A.	Obs.	Day.	Observed R.A.	Secs. of Hansen's R.A.	Diff. (H.−Obs.)	Corr. due to Error in Time.	Residual Error in R.A.	Observed N.P.D.	Secs. of Hansen's N.P.D.	Diff. (H.−Obs.)	Corr. due to Error in Time.	Residual Error in N.P.D.
s			h m s	s	s	s	s	° ′ ″	″	″	″	″
28·36	R.	Jan. 15	1 15 48·32	49·12	+0·80	−1·08	−0·28	79 31 19·00	16·22	−2·78	+5·05	+2·27
28·36	W.	16	2 13 2·94	3·83	+0·89	−1·10	−0·21	75 28 61·52	58·71	−2·81	+4·12	+1·31
28·37	F.B.	18	4 10 51·69	52·48	+0·79	−1·13	−0·34	70 22 5·83	5·94	+0·11	+1·59	+1·70
28·39	W.	23	8 53 4·01	4·95	+0·94	−0·97	−0·03	77 39 22·50	25·93	+3·43	−4·16	−0·73
28·40	R.	26	11 17 5·30	6·28	+0·98	−0·88	+0·10	89 56 22·27	26·05	+3·78	−4·91	−1·13
28·47	W.	Feb. 13	2 53 51·74	52·51	+0·77	−1·13	−0·36	73 27 58·92	57·11	−1·81	+3·39	+1·58
28·48	F.B.	15	4 52 40·49	41·40	+0·91	−1·13	−0·22	69 55 56·51	59·03	+2·52	+0·62	+3·14
28·48	R.	16	5 51 7·12	7·75	+0·63	−1·10	−0·47	70 0 9·52	9·91	+0·39	−0·75	−0·36
28·50	R.	19	8 34 44·81	45·41	+0·60	−0·98	−0·38	76 26 13·53	17·15	+3·62	−3·84	−0·22
28·51	W.	22	10 59 52·46	53·55	+1·09	−0·89	+0·20	88 13 28·20	32·28	+4·08	−4·89	−0·81
28·51	R.	23	11 46 6·56	7·43	+0·87	−0·89	−0·02	92 26 37·33	41·09	+3·76	−4·83	−1·07
28·62	R.	Mar. 23	12 16 36·41	37·31	+0·90	−0·89	+0·01	95 6 35·24	39·98	+4·74	−4·66	+0·08
28·64	R.	26	14 39 49·06	49·93	+0·87	−0·96	−0·09	105 29 38·11	39·28	+1·17	−3·09	−1·92
28·72	R.	April 16	9 41 53·88	54·29	+0·41	−0·93	−0·52	81 36 49 84	53·87	+4·03	−4·48	−0·45
28·74	R.	20	12 47 44·17	44·91	+0·74	−0·91	−0·17	97 45 42·66	45·89	+3·23	−4·44	−1·21
28·74	F.B.	21	13 35 1·44	2·31	+0·87	−0·93	−0·06	101 23 47·01	50·22	+3·21	−3·99	−0·78
28·76	W.	25	16 59 48·20	49·43	+1·23	−1·05	+0·18	109 42 25·96	23·16	−2·80	−0·41	−3·21

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Corr. to be subtracted from M.T. computed directly from Sid. Time at Mean Noon from N.A.	Obs.	Day.	Observed R.A.	Secs. of Hansen's R.A.	Diff. (H.−Obs.)	Corr. due to Error in Time.	Residual Error in R.A.	Observed N.P.D.	Secs. of Hansen's N.P.D.	Diff. (H.−Obs.)	Corr. due to Error in Time.	Residual Error in N.P.D.
s			h m s	s	s	s	s	° ′ ″	″	″	″	″
28·77	R.	April 29	20 40 28·25	29·31	+1·06	−1·07	−0·01	102 52 34·19	29·53	−4·66	+4·21	−0·45
28·84	R.	May 16	11 45 34·31	34·80	+0·49	−0·89	−0·40	92 25 4·68	7·87	+3·19	−4·79	−1·60
28·86	F.B.	22	16 42 15·19	16·35	+1·16	−1·05	+0·11	109 33 40·03	38·35	−1·68	−0·80	−2·48
28·87	R.	23	17 37 11·21	12·19	+0·98	−1·07	−0·09	109 44 11·88	8·67	−3·21	+0·40	−2·81
28·87	F.B.	24	18 32 45·93	46·92	+0·99	−1·08	−0·09	108 50 63·65	59·80	−3·85	+1·65	−2·20
28·95	W.	June 12	11 28 8·39	9·19	+0·80	−0·91	−0·11	90 44 11·16	16·02	+4·86	−4·88	−0·02
28·96	W.	14	13 1 0·04	1·01	+0·97	−0·92	+0·05	98 43 39·19	42·85	+3·66	−4·38	−0·72
29·09	W.	July 17	17 50 6·01	6·82	+0·81	−1·10	−0·29	109 36 27·27	26·01	−1·26	+0·61	−0·65
29·10	W.	19						106 19 9·74	5·84	−3·90	+3·19	−0·71
29·12	R.	26	2 18 46·18	47·32	+1·14	−1·14	0·00	76 6 60·21	54·30	−5·91	+4·17	−1·74
29·20	R.	Aug. 13	17 25 10·48	11·33	+0·85	−1·08	−0·23	109 33 1·72	1·06	−0·66	0·00	−0·66
29·20	W.	14	18 21 8·01	9·05	+1·04	−1·11	−0·07					
29·21	W.	16	20 15 29·52	30·64	+1·12	−1·12	0·00	104 38 34·36	30·52	−3·84	+3·78	−0·06
29·22	W.	18	22 9 57·41	58·83	+1·42	−1·12	+0·30	96 31 61·69	58·37	−3·32	+5·54	+2·22
29·23	W.	22	2 0 37·30	38·59	+1·29	−1·16	+0·13	77 30 41·25	37·62	−3·63	+4·56	+0·93
29·24	R.	23	3 0 24·64	25·63	+0·99	−1·18	−0·19	74 5 36·79	35·59	−1·20	+3·41	+2·21
29·32	F.B.	Sept. 13	20 44 25·51	26·54	+1·03	−1·12	−0·09	102 47 15·04	10·88	−4·16	+4·24	+0·08
29·33	W.	15	22 38 51·20	52·27	+1·07	−1·13	−0·06	94 4 60·31	55·44	−4·87	+5·81	+0·94
29·33	F.B.	16	23 36 54·29	55·65	+1·36	−1·15	+0·21	89 2 16·42	11·53	−4·89	+6·02	+1·13

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Corr. to be subtracted from M.T. computed directly from Sid. Time at Mean Noon from N.A.	Obs.	Day.	Observed R.A.	Secs. of Hansen's R.A.	Diff. (H.—Obs.)	Corr. due to Error in Time.	Residual Error in R.A.	Observed N.P.D.	Secs. of Hansen's N.P.D.	Diff. (H.—Obs.)	Corr. due to Error in Time.	Residual Error in N.P.D.
s			h m s	s	s	s	s	° ′ ″	″	″	″	″
29·34	F.B.	Sept. 17	0 35 55·09	56·64	+1·55	−1·17	+0·38	83 59 32·85	25·90	−6·95	+5·79	−1·16
29·44	R.	Oct. 12	22 9 14·29	15·15	+0·86	−1·11	−0·25	96 28 58·55	53·26	−5·29	+5·41	+0·12
29·44	W.	13	23 6 2·44	3·42	+0·98	−1·13	−0·15	91 41 18·16	14·39	−3·77	+5·89	+2·12
29·45	W.	16	2 6 30·62	31·92	+1·30	−1·24	+0·06	77 19 59·79	57·49	−2·30	+4·70	+2·40
29·46	F.B.	17	3 10 21·05	22·31	+1·26	−1·27	−0·01	73 52 45·45	43·07	−2·38	+3·38	+1·00
29·47	R.	21	7 20 23·45	24·59	+1·14	−1·13	+0·01	73 2 59·29	61·19	+1·90	−2·60	−0·70
29·55	R.	Nov. 9	22 39 12·09	12·89	+0·80	−1·09	−0·29	93 59 51·12	47·70	−3·42	+5·52	+2·10
29·55	F.B.	10	23 34 44·28	45·24	+0·96	−1·12	−0·16	89 12 8·58	5·17	−3·41	+5·81	+2·40
29·56	R.	12	1 32 14·97	15·81	+0·84	−1·21	−0·37	79 38 52·17	49·86	−2·31	+5·22	+2·91
29·58	R.	16	5 51 22·97	24·23	+1·26	−1·27	−0·01	70 54 47·79	47·27	−0·52	−0·64	−1·16
29·60	R.	22	11 18 56·86	57·89	+1·03	−0·94	+0·09	89 23 6·35	12·44	+6·09	−4·91	+1·18
29·65	W.	Dec. 5	21 27 18·78	19·71	+0·93	−1·05	−0·12	99 52 44·84	40·34	−4·50	+4·61	+0·11
29·68	F.B.	12	4 10 13·52	14·60	+1·08	−1·30	−0·22	71 46 9·08	8·69	−0·39	+2·03	+1·64
29·69	R.	14	6 21 8·05	9·14	+1·09	−1·27	−0·18	71 14 14·22	16·17	+1·95	−1·37	+0·58
29·69	F.B.	15	7 23 50·56	51·71	+1·15	−1·21	−0·06	73 1 6·26	8·70	+2·44	−2·80	−0·36
	Mean of errors without regard to sign ...				0·982		0·169			3·173		1·270
	Mean errors for year				+0·982		−0·095			−0·686		+0·096

Observers: W. = Mr. Wickham. R. = Mr. Robinson. F.B. = Mr. F. Bellamy.

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TABLE II.

Radcliffe Observations of the Moon, 1883. *Errors of Longitude and Ecliptic N.P.D.*

Day 1883.	Errors of Longitude. Corrected for Error in Time.	Uncorrected.	Errors of Ecl. N.P.D. Corrected for Error in Time.	Uncorrected.
Jan. 15	−4″·69	+11″·99	+0″·55	+1″·87
16	−3·31	+13·13	+0·23	+1·64
18	−5·04	+10·95	+0·79	+2·15
23	−0·63	+14·23	−0·58	−0·48
26	+0·94	+15·09	−1·63	−2·28
Feb. 13	−5·41	+11·13	+0·02	+1·46
15	−3·45	+12·45	+2·76	+4·00
16	−6·63	+ 8·84	−0·45	+0·51
19	−5·44	+ 9·41	+1·17	+1·33
22	+2·46	+16·69	−1·90	−2·54
23	−0·70	+13·52	−0·86	−1·74
Mar. 23	+0·17	+14·25	+0·01	−1·00
26	−1·83	+12·34	−1·43	−2·72
April 16	−7·46	+ 7·10	+2·12	+1·80
20	−2·80	+11·40	−0·13	−1·31
21	−1·11	+13·09	−0·41	−1·66
25	+2·20	+17·02	−3·45	−4·57
29	−0·03	+16·24	−0·47	−0·53
May 16	−6·16	+ 8·04	+0·92	+0·01
22	+1·21	+16·04	−2·67	−3·86
23	−1·38	+13·72	−2·86	−2·68
24	+1·40	+14·26	−2·27	−3·05
June 12	−1·53	+13·02	+0·63	−0·26
14	+0·41	+14·70	−0·95	−2·15
July 17	−4·12	+11·46	−0·58	−1·46
26	−0·57	+17·61	−1·64	−0·15
Aug. 13	−3·29	+11·98	−0·47	−1·37
16	+0·01	+16·79	−0·06	−0·10
18	+3·40	+21·03	+3·66	+4·39
22	+1·46	+18·99	+1·53	+3·11
23	−3·25	+14·04	+1·35	+2·87
Sept. 13	−1·30	+15·70	−0·27	−0·06
15	+0·48	+16·73	+0·54	+1·48

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Day 1883.	Errors of Longitude. Corrected for Error in Time.	Uncorrected.	Errors of Ecl. N P.D. Corrected for Error in Time.	Uncorrected.
	"	"	"	"
Sept. 16	+2·45	+20·74	+2·29	+3·57
17	+5·67	+24·00	+1·16	+2·71
Oct. 12	−3·54	+13·91	−1·21	−0·41
13	−2·90	+15·05	+1·08	+2·22
16	+0·01	+18·68	+2·56	+4·30
17	−0·41	+18·15	+0·92	+2·60
21	+0·05	+16·52	−0·71	−0·35
Nov. 9	−4·83	+12·42	+0·32	+1·32
10	−3·17	+14·62	+1·26	+2·55
12	−6·15	+12·38	+0·71	+2·39
16	−0·13	+17·92	−1·16	−0·27
22	+1·71	+16·68	+0·56	−0·45
Dec. 5	−1·72	+14·51	−0·45	+0·05
12	−3·39	+15·25	+1·04	+2·45
14	−2·54	+15·58	+0·68	+1·36
15	−0·91	+16·74	−0·23	+0·05
Mean of errors without regard to sign	2·528	14·615	1·137	1·789
Mean errors for year ...	−1·547	+14·615	+0·041	+0·342

Table III.

Observations of the Moon, 1862 to 1883. Uncorrected and Corrected for Error in Time.

Hansen — Observed.

Year.	R.A. Uncorrected.	R.A. Corrected.	N.P.D. N.P.D. Increasing Uncorrected.	N.P.D. Increasing Corrected.	N.P.D. Decreasing Uncorrected.	N.P.D. Decreasing Corrected.
	s	s	"	"	"	"
1862 Gr.	−0·191	−0·191	+0·00	+0·00	+0·02	+0·02
1863 „	−0·129	−0·129	+0·04	+0·04	−0·27	−0·27
1864 „	+0·001	−0·048	−0·18	−0·32	−0·53	−0·34
1865 „	+0·085	−0·019	−0·33	−0·60	−0·89	−0·53
1866 „	+0·135	−0·020	+0·62	+0·16	−0·53	0·00
1867 „	+0·235	+0·028	+0·80	+0·26	−0·35	+0·36
1868 „	+0·275	+0·015	+1·26	+0·36	−0·74	+0·07
1869 „	+0·295	−0·016	+0·94	−0·15	−1·05	+0·06
1870 „	+0·329	−0·044	+0·92	−0·40	−1·44	−0·12

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Year.	R.A.		N.P.D.			
			N.P.D. Increasing		N.P.D. Decreasing	
	Uncorrected.	Corrected.	Uncorrected.	Corrected.	Uncorrected.	Corrected.
1871 Gr.	+0·448	+0·019	+1·93	+0·15	−2·49	−0·70
1872 „	+0·478	−0·002	+2·16	+0·07	−2·93	−0·64
1873 „	+0·522	−0·008	+3·09	+0·75	−3·11	−0·46
1874 „	+0·597	+0·026	+3·28	+0·64	−3·53	−0·44
1875 „	+0·649	+0·026	+3·03	+0·03	−3·40	−0·15
1876 „	+0·637	−0·040	+4·11	+0·57	−3·05	+0·73
1877 „	+0·585	−0·146	+4·76	+0·68	−2·95	+0·54
1878 „	+0·530	−0·251	+4·47	+0·22	−2·29	+1·32
1879 „	+0·613	−0·241	+4·25	+0·41	−3·32	+0·48
1880 „	+0·663	−0·232	+3·66	−0·27	−3·44	+0·34
1881 „	+0·687	−0·266	+3·30	−0·40	−3·98	0·00
1882 „	+0·82					
1882 Rad.	+0·858	−0·162	+2·73	−0·83	−3·44	+0·70
1883 „	+0·982	−0·095	+2·60	−0·81	−3·06	+0·75

On the Proper Motions of Forty Stars in the Pleiades, both Absolute and Relative. By Prof. C. Pritchard, D.D., F.R.S.

(*Abstract.*)

The Memoir which I am about to present to the consideration of the Society, consists mainly of two separate and entirely independent investigations of the proper motions of forty stars in the *Pleiades*. It originated primarily in a desire to test the capacity of a new Differential Micrometer which I had devised, under the expectation that it would enable me to measure with great accuracy celestial arcs to the extent of some twenty minutes; a distance which seemed to me considerably greater than could be reached with security by means of any filar micrometer with which I was acquainted. I purposed to apply this new form of micrometer, if successful, to a systematic investigation of stellar parallax, which I then intended to make an especial object of research, in the observatory under my direction at Oxford. I applied the instrument in the first instance to the *Pleiades*, in consequence of a remark* made by Bessel, to the effect that his own measures of the relative coordinates of certain members of this historic group had been effected with so much accuracy by means of his heliometer, that advantage might henceforth be taken of his scrupulous labours therein, to test the accuracy of other instruments, and, at the same time, assist in determining their scales.

I had proceeded to a considerable extent in this investigation

* Beobachtungen verschiedener Sterne der Plejaden, 'Astronomische Untersuchungen,' erster Band. S. 209.

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when, becoming satisfied with the capacity of the new micrometer, I resolved to still further utilise the observations by widening the scope of the work, and by embarking on a systematic measurement of the relative positions of the more prominent members of the group, with the view of ascertaining their relative proper motions since the time of Bessel's heliometer measures made some forty years ago.

The instrument in question affords the means of measuring arcs with equal facility in the three coordinate directions of Right Ascension, Declination, and Distance. Consequently, if all three were measured, there would arise at once the means of testing the accuracy of the observations. This seemed to me to be a great gain, and I have uniformly applied this test to the observations recorded in the present Memoir.

Such, then, are the circumstances under which the research commenced. As I proceeded with it, and endeavoured to make it complete, the questions before me widened out, and new objects of research presented themselves in close connection with the original inquiry. It became desirable for instance to record the relative magnitudes, or degrees of lustre, presented by the members of the group subjected to the micrometer. Estimations made by the eye on these objects as viewed in the field of the telescope, seemed to me to be eminently unsatisfactory. In support of this remark, I may mention that out of estimations made by eminent observers, such as Flamsteed, Bradley, Mayer, Piazzi, Bessel, Argelander, and Wolf, no two agree in the order of the relative brightness of the eight principal stars in the cluster.* These considerations led me to the invention of the photometric method of measurement of relative brightness by means of the '*Wedge Photometer*,' and this has already appeared in the Memoir on the subject, printed in the last volume of the Society's Transactions, and which I trust, before long, to complete as a new Uranometria of all the stars visible to the naked eye from the Pole to the Equator.

In the course of the reduction of the micrometer measures another question also not unnaturally arose. The results thus obtained are from *differential* observations. But scattered about in the records of many observatories, from the time of Bradley downwards to the present time, there are numerous observations of several members of the group, made with meridian instruments, and affording reliable measures of their absolute Right Ascensions and Declinations. If therefore these valuable observations, made at different epochs and by various observers of great eminence, were reduced on a uniform and systematic plan, it would thereby be possible to deduce the present coordinates and the proper motions of the objects to which these observations refer. The work, I felt, would be somewhat laborious; but it would supply the means of an interesting and

* Description du groupe des Pléiades, par M. C. Wolf, p. A 24.

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somewhat novel intercomparison between the results obtained by a long series of meridian observations, and those accruing from differential measures, made away from the meridian. Accordingly I was led to undertake the task, the numerical results of which are now complete and ready for presentation to the Society.

There are fourteen stars in the *Pleiades* whose places have been sufficiently obtained by meridian observations. They extend for the most part through a period of one hundred and thirty years, and are worthy of various and perhaps of determinable degrees of reliance. In systematically effecting their reduction, I have followed and somewhat extended the plan pursued by Argelander in the seventh volume of the Bonn observations, largely availing myself of the facilities afforded by the later labours of Professors Newcomb and Boss. The result of this section of the present research, consists in the determination of the most probable absolute coordinates and proper motions of the fourteen stars in question. These proper motions thus obtained from the best existing meridian observations, are then compared with those resulting from micrometrical measurements. For the latter purpose recourse is had to the heliometer measures made at Königsberg between 1838–41; to a very valuable set of differential measures made at Paris by Dr. Wolf in 1874; and to the measures made at Oxford with the 'Duplex Micrometer' between 1878–1880.*

These three sets of observations are combined together by means of equations of condition; and, as in the case of the forementioned meridian work, furnish at once the most probable relative coordinates of the forty stars in question, together with their relative proper motions.

The final results of these investigations are as follows:— First, with regard to the fourteen stars which have been measured by the two thoroughly independent processes; the general accord of the final results points unmistakably to the existence of certain small motions of these stars *inter se*; but of so slight an amount, that their exact determination does not seem fully within the reach of observations as yet extant; these must be left for the effects of a greater lapse of time, which shall make them more directly apparent in amount, and to the astronomy of the future. Of the remaining stars, although meridian observations do not at present exist so as to corroborate the evidences of relative motion afforded by the combined micrometer measures made at Königsberg, Paris, and Oxford, nevertheless the fact of these minute displacements is, I think, now established beyond

* In addition to these, there is another apparently valuable set of measures made at Washington about the year 1860. These observations, however, do not appear to have been systematically reduced. In the Washington Catalogue for 1860, the absolute coordinates are given, but the information there supplied is, I regret, not sufficient to enable me to combine them with the other series of coordinates here mentioned.

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doubt. An independent confirmation of these results is also obtained from a comparison of '*distance*' measured at Königsberg and at Oxford.

Another remark also occurs, which is this: the relative displacements of these distant suns, although not distinctly and accurately measurable in numerical extent, appear to vary both in direction and amount; indicating thereby the mutual influence of a group of gravitating bodies, and not simply that common motion of the whole which would necessarily arise simply from the translation of the solar system in space.

In further connection with the completion, for the present, of this very interesting inquiry into the observable motions of a cosmical system, I may take this opportunity of stating, that I have laid the foundation for a similar enquiry, destined for the astronomers of the future, by making with scrupulous care, in the University Observatory, a series of observations of the relative positions of some 250 stars of a cluster in *Cygnus* (39 Messier), which I hope, in due time, to communicate to the Society. Perhaps these measures, after the lapse of a century or more, may, like Bradley's observations, made 130 years ago, again serve to trace the effects of gravitation, in systems of suns, possibly detached from our own.

Oxford: 1884 *May* 8.

Observations made at the U.S. Naval Observatory, Washington. By Prof. Asaph Hall.

(*Communicated by Commodore S. R. Franklin, U.S.N., Sept.*)

CONJUNCTION OF THE SATELLITES OF SATURN.

Mimas.

Date.		Wash. M.T. h m	Position.	Wt.	Remarks.
1883 Dec.	3	10 55·8	S.p.	3	
	3	12 10·8	S.p.	3	
	6	9 19·9	S.	3	
1884 Jan.	26	6 40·8	S.	3	
Feb.	1	9 46·5	N.	3	

Enceladus.

1883 Dec.	6	9 44·4	N.	3	
	17	8 42·3	N.	3	
	28	7 45·9	N.	2	
1884 Jan.	21	7 6·7	S.	3	
Feb.	1	6 18·0	S.	3	
	18	9 25·6	N.	3	
	29	8 35·1	N.	3	

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Tethys.

Date.		Wash. M.T.	Position.	Wt.	Remarks.
		h m			
1883 Dec.	15	10 40·2	S.	2	Cloudy
	17	7 59·3	S.	3	
1884 Jan.	2	8 50·5	N.	2	
	3	7 35·5	S.	2	Very faint, hazy
	4	6 8·1	N.	3	
	20	7 15·6	S.	3	
	21	5 54·7	N.	3	
Feb.	21	9 35·8	S.	2	

Dione.

1883 Nov.	4	11 12·7	N.	3	
Dec.	11	9 18·1	S.	1	Strong moonlight
1884 Jan.	6	9 0·8	N.	3	
	17	7 44·7	N.	3	
Feb.	1	9 3·0	S.	3	
	23	6 35·8	S.	2	Cloudy

Rhea.

1883 Dec.	17	10 0·3	N.	3	
1884 Jan.	20	6 38·1	S.	3	

Observations of the Satellites of Mars.

Phobos.

Date.		Wash. M.T.	p.	Wt.	Wash. M.T.	s.	Wt.	Remarks.
		h m	°		h m	″		
1884 Jan.	26	9 59·8	95·1	3	10 12·3	20·35	3	Very faint
Feb.	1	11 12·0	90·1	2	11 20·5	19·70	2	Very faint

Deimos.

1884 Jan.	22	11 32·7	89·8	2	...	...	...	Clouds
	25	13 17·3	263·25	3	13 23·3	46·43	3	
	25	14 39·3	268·45	2	14 45·8	49·23	2	
	26	8 33·3	97·89	3	8 39·3	41·52	3	Faint
	29	9 35·9	268·65	3	9 41·9	48·80	3	
	29	10 54·4	272·03	3	11 0·9	47·25	3	Faint
Feb.	1	11 33·0	82·21	2	11 40·0	45·08	2	Faint
	15	9 8·0	80·44	3	9 18·0	45·35	3	Faint
	15	10 19·5	84·78	3	10 30·5	45·75	3	
	20	10 58·7	82·51	2	11 9·2	44·73	2	Very faint

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COMPARISON WITH MARTH'S EPHEMERIS.

Phobos.

		(O−C) Δp	Δs
		°	″
Jan.	26	−4·4	−1·44
Feb.	1	−3·0	−0·36

Deimos.

Jan.	22	+1·68	...
	25	+1·21	−0·86
	25	+0·58	−1·13
	26	+0·33	+0·89
	29	+0·02	−0·46
	29	+0·70	−0·29
Feb.	1	+0·34	+0·42
	15	+1·63	+0·01
	15	+0·53	+0·56
	20	+0·77	+0·19

OBSERVATIONS OF THE COMPANION OF SIRIUS.

Date.	Sid. Time.	*p.*	*s.*	Wt.	Remarks.
	h	°	″		
1884·170	5·6	39·0	8·89	2	Very faint
·206	6·1	38·0	8·76	2	Faint
·209	6·2	37·5	8·64	2	Faint
·225	6·8	36·5	8·76	2	Very faint
·231	6·7	37·5	8·87	3	
·247	7·3	37·8	8·95	2	Faint
·250	7·4	37·2	8·87	2	
·269	7·9	37·8	8·74	3	

MEAN RESULTS.

	p.	*s.*
	°	″
1884·226	37·67	8·810

In observing the conjunctions of the satellites of *Saturn* the wire of the micrometer was set perpendicular to the major axis of the ring by means of the angle given in the American *Ephemeris*, p. 465, and the time was noted when the satellite was bisected

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by the wire. The satellite was watched several minutes before and after the conjunction. On Dec. 3 *Mimas* was observed in conjunction with the preceding end of the Cassini division of the ring, and again when in conjunction with the preceding edge of the ball. On good nights, with the present opening of the ring, this satellite can be followed to conjunction with ease and certainty.

The two observations of the inner satellite of *Mars* were made with difficulty, and these observations are of little value except to show that the elements of this satellite are nearly correct. The outer satellite could be observed accurately for about one month when near opposition. It could be followed longer with our 26-inch glass, but it is hardly worth while to do this, and thus bring in a quantity of uncertain observations. In both cases the residuals in the angles indicate small corrections to the periods of these satellites, but these periods are no doubt accurate enough to carry forward the Ephemerides for several succeeding oppositions of the planet. As the motion of these satellites in angle of position is now direct, or different from what it was at the time of discovery, it seems better to wait for further observations—at least during one favourable opposition—before making a final discussion of the observations. The observations above are corrected for differential refraction, and for the figure of the planet.

I am much indebted to Mr. Marth for the Ephemerides of satellites that he publishes.

The companion of *Sirius* has been very faint and difficult to observe during the present year. This was owing chiefly, I think, to the very unfavourable weather that has prevailed. The rainfall at Washington has been about 21 inches during the first three months of the year.

The Motion of Hyperion. By Prof. Asaph Hall.

In the *Astronomische Nachrichten*, Nos. 2246 and 2263, I have called attention to the motion of this satellite of *Saturn*, and to the difficulties that prevent an accurate determination of this motion. These difficulties arise in part from the fewness of the observations made from the time of its discovery in 1848 until 1875. In fact, during this interval of twenty-seven years the only complete series of observations is that made by Mr. Lassell at the opposition of 1852. For this reason the mean motion of this satellite is not yet well known, and it appears that we must wait for still further observations; but the following results may be of some interest, and may induce some astronomer who has the means, to assist in observing this faint object.

Denoting by s the observed distance of the satellite from the

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centre of *Saturn*, and by p the corresponding angle of position, the formulæ of Marth give

$$r \sin (H+u) = \frac{\rho}{\rho_0} s . \frac{\cos (p-k)}{\cos k}$$

$$r \cos (H+u) = \frac{\rho}{\rho_0} s . \sin (p-k)$$

In these formulæ r is the radius vector of the satellite, and u is the argument of the latitude, ρ is the distance of the planet from the Earth, and ρ_0 its mean distance from the Sun. The quantities h, k, and cos h depend on the position of the orbit plane of the satellite, and the Right Ascension and Declination of the planet; and hence these quantities may be tabulated when the inclination and node of the orbit plane with respect to the equator are given. The plane of the orbit of *Hyperion* coincides nearly with that of *Titan*, and its position might be assumed the same, but I have used my former determination of the node and inclination which does not differ much from those of *Titan*. The quantities h, k, and cos h were then tabulated for each of the oppositions 1875 to 1883, and the values of r and u were computed from the observed values of s and p. Then having the radii vectores and the angles u, which vary nearly as the true anomalies, the elements of the satellite in the plane of its orbit were found. The following table gives the results of this calculation. The first column gives the year of the opposition, and I have included Mr. Lassell's observations of 1852. The second column gives the number of equations of condition. Each observation furnishes an equation of the form

$$x + by + cz + n = 0,$$

where

$$b = r \cos u,\ c = r \sin u,\ n = p_0 - r,$$
$$x = \Delta p_0,\ y = -e \cos (N-P),\ z = e \sin (N-P);$$

and p_0 is an assumed value of the semi-parameter, N and P are the longitudes of the node and peri-saturnium on the equator, and e is the eccentricity of the orbit. These equations solved by the method of least squares gave the quantities in the following columns. The third and fourth columns give the resulting values of the semi-major axis, and the eccentricity of the orbit. The fifth column gives the value of the longitude of the peri-saturnium with respect to the equator. In the last column is given the average value of cos h. It will be seen by the formulæ that cos h enters into the value of $r \sin (H+u)$, and when this cosine is very small the values of r and u may be largely affected by small errors of observation. For this reason I have omitted the years 1877 and 1878, when the value of cos h was less than one-tenth.

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Date.	No. of Equations.	a	e	P	cos h.
1852·9	30	217″·05	0·1201	240°·182	0·33
1875·7	32	216·25	0·1026	174·242	0·23
1876·7	11	216·52	0·1290	156·922	0·15
1879·8	27	211·17	0·0780	93·173	0·13
1880·9	48	212·41	0·0823	60·012	0·23
1881·9	42	213·05	0·0898	36·908	0·30
1882·9	15	215·46	0·0884	20·043	0·37
1883·9	30	212·90	0·0982	353·272	0·41

This table shows a very large change in the value of P. These values may be represented very nearly by the formula

$$P = 174°\cdot242 - 20°\cdot344t - 0°\cdot10254t^2$$

t denoting the number of Julian years from the epoch 1875–7.

There is yet some uncertainty in the mean motion of *Hyperion*, but the best value is found probably by comparing Mr. Lassell's observations of 1852 with the recent ones. A comparison of these observations with my own of 1880 and 1882 gives the following value of the periodic time:

Sidereal Period = 21·276742 days;

and the synodical period is 21·318901 days.

On account of its mean distance and the eccentricity of its orbit *Hyperion* can approach very near to *Titan*; and since three times the period of one is nearly equal to four times the period of the other, there is probably a large perturbation of the motion of *Hyperion* by *Titan*, depending on the first powers of the eccentricities of their orbits. With the above value of the mean motion of *Hyperion* this perturbation will have a period of 19·185 years. The complete calculation of such a perturbation would be laborious in this case, since in computing the $b_s^{(i)}$ coefficients of Laplace we shall have

$$\alpha = \frac{176\cdot552}{213\cdot98} = 0\cdot8250863,$$

and the series for these coefficients converge slowly.

Also a small change in the mean motion of *Hyperion* would have a large influence on the values of the coefficients in the expressions for the perturbations, and for this reason it does not seem worth while to give much time to such a work at present. But, in order to form some idea of the action of *Titan*, I have computed the coefficients in the perturbations of the radius vector and the true longitude, including the first powers of the eccentricities by the formulæ of the *Mécanique Céleste*, tome i. pp. 279, 280. In computing the values of $b_1^{(i)}$ and the derivatives, no attempt was made to carry the numerical approximations very far. Distinguishing the elements of *Titan* with an accent we have

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$$a = 213''.98 \qquad a' = 176''.552$$
$$e = 0.1 \qquad e' = 0.029223$$
$$\tau = 21.276742 \text{ days} \qquad \tau' = 15.94545154 \text{ days}$$
$$m' = \text{mass of Titan.}$$

$$\delta r = \left\{ \begin{array}{l} -0.32865 - 4.664 \cdot \cos(n't - nt + \epsilon' - \epsilon) \\ \qquad - 2.902 \cdot \cos 2(n't - nt + \epsilon' - \epsilon) \\ \qquad + 183.075 \cdot \cos 3(n't - nt + \epsilon' - \epsilon) \\ \qquad + 0.114 \cdot \cos 4(n't - nt + \epsilon' - \epsilon) \\ am'_0 \left\{ \begin{array}{l} + 0.407 \cdot \cos(nt + \epsilon - \pi) - 0.1607 \cdot \cos(nt + \epsilon - \pi') \\ + 6.332 \cdot t \sin(nt + \epsilon - \pi) - 2.0570 \cdot t \sin(nt + \epsilon - \pi') \\ - 166.367 \cdot \cos[(4n - 3n') \cdot t + 4\epsilon - 3\epsilon - \pi] \\ + 16.814 \cdot \cos[(4n - 3n') \cdot t + 4\epsilon - 3\epsilon' - \pi'] \end{array} \right. \end{array} \right.$$

$$\delta v = \left\{ \begin{array}{l} + 30.065 \cdot \sin(n't - nt + \epsilon' - \epsilon) \\ \qquad + 5.739 \cdot \sin 2(n't - nt + \epsilon' - \epsilon) \\ \qquad - 366.667 \cdot \sin 3(n't - nt + \epsilon' - \epsilon) \\ \qquad - 0.932 \cdot \sin 4(n't - nt + \epsilon' - \epsilon) \\ m'_0 \left\{ \begin{array}{l} - 12.665 \cdot t \cos(nt + \epsilon - \pi) + 4.114 \cdot t \cos(nt + \epsilon - \pi') \\ - 73156.67 \sin[(4n - 3n')t + 4\epsilon - 3\epsilon' - \pi] \\ + 8307.50 \sin[(4n - 3n')t + 4\epsilon - 3\epsilon' - \pi'] \end{array} \right. \end{array} \right.$$

The large coefficients in the perturbation of the longitude indicate that a small mass of *Titan* may have a great influence on the motion of *Hyperion*. But the mean motion of this last satellite must be known more exactly before any inference can be drawn. It is possible that the quantity $4n - 3n'$ may prove to be extremely small, so that we may have here a relation similar to that which exists among the satellites of *Jupiter*. Should such be the case the above values of δr and δv would be of little use, and the theory would be greatly changed. The mass of *Titan* is known only from estimates of its diameter, and by assuming its density. On several occasions during the past eight years, when the images were good, I have estimated the apparent diameter of *Titan* to be $0''.75$; but looking at some of the fixed stars on the same nights these also appeared to have small disks, and hence I am distrustful of diameters estimated in this way. According to Professor Pickering's photometric measurements, and assuming the densities of these satellites to be the same, we have

Mass of Titan = 387 times mass of Hyperion.

It is possible now, I think, to make a tolerably accurate ephemeris of *Hyperion* for the next opposition, but the one given in the American *Ephemeris* will serve very well for observing the satellite. It should be stated, however, that during the

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opposition of 1883, *Hyperion* came to conjunction about one day sooner than the times given in that *Ephemeris*.

The observations of *Hyperion* made by Mr. Lassell during the opposition of 1852 are important, since they form the earliest complete series that we have. These observations are published in the *Monthly Notices*, vol. xiii. p. 181. I hope that Mr. Marth or some one familiar with Mr. Lassell's method of work, may be willing to revise the reductions and publish the observations with more detail.

1884 *April* 10.

Observations of the Companion of Sirius, made at the Dearborn Observatory, Chicago, U.S.A. By Prof. G. W. Hough and S. W. Burnham.

Date.	*p.*	*s.*
	°	″
1884·025	37·9	8·60
·057	36·7	8·35
·170	37·0	8·64
·186	37·1	8·39
·197	36·5	8·78
·200	36·5	8·58
·203	36·9	8·37
·217	37·0	8·51
·225	36·1	8·51
·233	35·9	8·30
·252	35·9	8·58
Mean = 1884·179	36·7	8·51

(*Observations by S. W. Burnham.*)

Date.	*p.*	*s.*
	°	″
1884·057	37·2	8·60
·167	39·3	8·33
·184	33·2	8·56
·195	36·4	8·11
·197	35·3	8·50
·200	35·8	8·31
·214	37·0	8·34
·217	36·3	8·22
·230	37·9	8·44
·233	35·5	8·51
Mean = 1884·19	36·4	8·39

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Note on a Method of Reducing the Friction of the Polar Axis of a Large Telescope. By A. A. Common.

In large telescopes the friction of the polar axis is generally reduced by means of wheels placed so as to take from off the upper and lower bearings the greater part of the total weight. The points or lines of contact between those wheels and the axis are very small, and the least inequality in shape or in the hardness of the metal must produce for slow motions an extremely irregular action.

The plan of floating off some of this weight, and so reducing the friction, has been employed by me in the construction of an 18-inch and a 3-foot Reflector, but in both cases only in a partial manner.

In the account I gave (in vol. xlvi. of the *Memoirs* of this Society) of the mounting of this latter telescope, I mentioned that this principle could be applied in a very complete manner by so arranging matters that the centre of gravity of the moving parts should be over the centre of flotation.

I had then considered several ways of doing this. Two of these I now propose to mention more particularly, and I have little hesitation in doing so, as the working of the 3-foot telescope has been so good in this respect as to make me think that the complete application of this principle would be the best way we could have of ensuring the proper and equable motion of the telescope in time. The first plan, arranged for the 3-foot, mounted on what is known as the German form of mounting, is simple. It is to enlarge the diameter of the polar axis at the upper part by a drum or cylinder of cast iron, of such a diameter and depth as to get the requisite displacement of mercury contained in an outer cylinder surrounding the drum spoken of. Altering the figures to suit an object-glass of, say, 40 inches aperture and of about 60 feet focal length (as the largest that may be at present thought of), we should have as the weight brought down to the centre of the declination axis something like six tons. This could be dealt with effectively by making the drum on the polar axis about 6 feet diameter and 3 feet deep, something less than one-half of this being immersed. These figures are, of course, merely approximate, but if the size and weight of the object-glass be known, the rest can all be easily calculated.

Here, from the low position of the centre of gravity of the whole and its proximity to the upper bearing of the polar axis, it is possible to get the centre of gravity and the centre of flotation in a vertical line in the way indicated.

This plan was not used: the form of mounting determined on as regards that part above the polar axis required that the centre of gravity of this part should be some distance from the upper bearing; the following arrangement was then planned to meet this requirement, but it also was not used, as it involves

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greater bulk than that finally adopted, and is only applicable to a permanent position.

In this the liquid proposed to be used is water or oil, the polar axis being made very much larger to get the necessary displacement, not only to carry itself and the weight of the telescope, but also to carry such a weight at the lower part as would bring the centre of gravity of the whole in such a position to the centre of flotation of the immersed part as to cause the axis to float at the required angle. For example, if we take the case of an 8-foot mirror, with a mounting for the upper part the same as my 3-foot telescope, we should have about nine tons, to be supported at a distance of about 8 feet from the upper bearing. If we now make a polar axis of boiler-plate about 50 feet long and 8 feet mean diameter, cylindrical, or preferably conical, and immerse this in water, we should be able by adding weight at the lower part to get such an arrangement that if the whole were placed in water it would float at the angle required, with the upper bearing above the water; it would then be only necessary to provide proper bearings for top and bottom, and add sufficient weight to ensure contact of the axis, and moor it, as it were, in its proper place, to complete an arrangement that would be, in my opinion, the best possible for the purpose. The weight we have taken as concentrated at the point 8 feet above the upper bearing can, of course, represent any form of optical arrangement that it is required to carry round, as, for instance, an Equatorial coudé; in this case the application would be easy, as the weights could be placed lower, and a continuation of the polar axis, of a similar but lighter construction, would form the tube and dispense with the upper bearing and pier otherwise needed. Or an arrangement for the use of a concave mirror, fixed at right angles to the polar axis at a distance something less than its focal length, with a plane mirror opposite to it similar in proportionate size, angle, position, and arrangement for turning, to the first mirror of the Equatorial coudé, the light of an object reflected from this plane mirror on to the concave mirror would be sent by this to a plane mirror placed, as in the Newtonian telescope, and fixed where the line joining the centres of the plane and concave mirrors, cuts the central line of the polar axis, from which it would pass out in the direction of this latter line, forming an image above, where it could be conveniently viewed or used.

The chief difficulty here would be the large plane mirror, but from what has been recently done, this may not be even a difficulty. If the plane mirrors can be made as easily and as well as the concave ones, such a plan of mounting allows them to be mounted in a very cheap and effective manner, and quite does away with the costly house or covering, and yet keeps all the advantages that other forms give.

May 1884.

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Note on Dr. N. de Konkoly's Remarks on Mr. Hilger's Illumination of Micrometers with Vacuum Tubes. By Rev. S. J. Perry, F.R.S.

The adverse criticism of Dr. Konkoly appears to arise from a mistake. The micrometer exhibited at the meeting of this Society differs entirely in the method of illumination from the instrument at the Observatory of Kalocsa. The latter was constructed in 1880, and has a side illumination, whereas the curved vacuum tube in the Vizagapatam micrometer is placed inside the micrometer, and gives uniform illumination of the whole system of webs.

The whole of the Equatorial, including micrometer and circles, may be most conveniently illuminated by a couple of bichromate cells, with wires that may be attached as required to fixed vacuum tubes. Such a system of illumination has been in use for some time at one of the private observatories of Lancashire, and is found to answer perfectly. That Dr. de Konkoly can obtain equally satisfactory results with a number of small Swan lamps is a matter of congratulation.

Stonyhurst Observatory:
1884 *May* 7.

The Orbit of Pons' Comet. By J. Morrison, M.A., M.D., Assistant on the American *Ephemeris and Nautical Almanac*, Washington, D.C.

The only orbits of this comet which have been published are those of the late Prof. Encke and of Messrs. Schulhof and Bossert: the former deduced from observations extending over a period of only two months in the year 1812, and the latter from observations made in 1883, and subsequently corrected two or three times by equations of condition. Neither Encke's orbit nor that of Schulhof and Bossert represents the motion of the comet with that degree of accuracy which modern investigations demand. In order to obtain an independent determination of the elements, I select the three following observations made at Washington:—

Washington M.T.	Apparent α	Apparent δ
1883 Oct. 10·307425	248° 25′ 56″·1	+56° 51′ 49″·0
Dec. 27·249330	317 34 50·4	+29 31 14·8
1884 Jan. 21·299233	357 36 1·5	−13 31 23·1

The comparison stars were respectively, a *Berliner Jahrbuch*

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star; *ζ Cygni*; and a 7th-magnitude star in *Cetus*, as given in the Catalogues of Weisse, Lamont, and Stone.

From these observations I obtain the following system of elements:—

T	... ...	1884 Jan. 25·729107, G.M.T.
ω	... ...	199° 12′ 50″·48 } Mean Equinox
Ω	... ...	254 9 45·82 } of 1884·0.
i	... ...	74 2 0·73
e	... ...	·95414506
log *q*	... ...	9·8896779
log *a*	... ...	1·2282918
P	69·572 years.	Motion Direct.

The residuals for the middle place are zero. It will be noticed that these elements differ considerably from those of Encke and Messrs. Schulhof and Bossert. The following residuals show how closely this system of elements satisfies the observations made from Sept. 5, 1883, to Feb. 5, 1884:

Date. Washington M.T.		O−C cos β dλ	O−C dβ	log Δ	Observation made at
1883 Sept.	5·11 +	− 3″·57	−3″·72	0·3668189	Milan.
	23·32 +	− 3·65	−5·09	0·3282883	Washington.
Oct.	31·29 +	− 6·14	−6·59	0·2103271	,,
Nov.	1·33 +	+ 3·73	−6·90	0·2060407	,,
	27·29 +	+ 4·82	+7·90	0·0707879	,,
Dec.	6·25 +	+10·03	−1·18	0·0093336	,,
	21·24 +	+10·60	−7·20	9·8932833	,,
1884 Jan.	3·23 +	− 3·12	−2·29	9·8126678	,,
	9·30 +	− 0·26	−1·48	9·8023359	,,
	17·30 +	− 0·82	−4·30	9·8246773	,,
	25·29 +	− 1·12	+1·10	9·8749017	,,
Feb.	1·28 +	+ 1·78	+0·14	9·9258903	,,
	5·01 +	− 2·74	+1·23	9·9522952	Rome.

Owing to errors in the adopted position of the comparison stars, many of which have not been very accurately determined, and also to the uncertainty always existing in observations of comets in consequence of the ever varying form of the nucleus, these residuals are less than what we may reasonably expect to arise from these causes alone, not to mention the unavoidable errors of computation, even when seven-figure tables are employed, as in the present instance. The formation of equations

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of condition for the purpose of correcting these elements, cannot be here applied with any advantage.

The only planets which can have any appreciable influence on the motion of the comet during the time it was in the neighbourhood of perihelion are *Venus* and the Earth. Assuming the elements I have obtained above as the osculating elements for the epoch Jan. 26·0, Greenwich Mean Time, the perturbations of the rectangular co-ordinates, arising from the action of *Venus*, are found to be as follows :—

Greenwich M.T.	δx	δy	δz
1884 Jan. 26·0	−0·0	− 0·0	+0·0
31·0	−0·1	− 0·3	+0·1
Feb. 5·0	−0·2	− 1·0	+0·4
10·0	−0·4	− 2·0	+0·9
15·0	−0·6	− 3·2	+1·7
20·0	−0·9	− 4·7	+2·6
25·0	−1·3	− 6·5	+3·8
Mar. 1·0	−1·7	− 8·5	+5·0
6·0	−2·1	−10·8	+6·5
11·0	−2·6	−13·5	+8·0

which are expressed in units of the *seventh* decimal place. These perturbations do not sensibly affect the inclination, and their influence on the node is less than a second of arc ; they are then almost inappreciable, and, indeed, they scarcely exceed the uncertainty of the seventh decimal place in the final results of extended computations such as the one now under consideration. The perturbations of the rectangular co-ordinates arising from the action of the Earth are even less than those produced by *Venus*, by reason of the Earth being at a greater distance from the comet.

According to my elements, the aphelion distance is 33·056, which exceeds *Neptune's* mean distance from the Sun by about *three* mean radii of the Earth's orbit, and although the inclination is large (74° 2′), the comet will nevertheless be sensibly affected by the action of all the superior planets during the greater part of its period. The amount of this action during the last revolution may, perhaps, be approximately found by comparing my elements with those of Encke, as given in the *Annales de l'Observatoire de Bruxelles*, tom. I. If we refer Encke's elements to the mean equinox and ecliptic of 1884·0, and place them beside mine in order to facilitate comparison, we have the following results :—

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	Morrison.	Encke.
T*	1884 Jan. 25·72917	1812, Sept. 15·313553
ω	199° 12′ 50″·48	199° 17′ 5″·89
Ω	254 9 45·82	254 1 28·91
i	74 2 0·73	73 56 57·41
e	0·95414506	0·9545412
log q	9·8896779	9·8904995
P	69·572 years	70·684 years

Assuming these values of T to be correct, the actual period of the last revolution was 71·3605 years, and therefore Encke's period has been increased by 247·09 days. It is very probable, however, that Encke's elements are considerably in error, for Messrs. Schulhof and Bossert have deduced from the original observations of 1812 the following elements, which agree tolerably well with mine :—

$\pi - \Omega = \omega$	199° 12′ 32″·5	Mean Equinox, 1880·0
Ω	254 6 15·3	
i	74 3 20·4	
e	0·9549960	
log q	9·8893650	

If we add 3′ 20″·92, or four years' precession, to Ω, we get 254° 9′ 36″·2, which is almost identical with my value, while the values of ω and i will not be sensibly affected by this change.

* Greenwich Mean Time.

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Sextant Observations of Comet Pons-Brooks, as seen from the ship "Superb," Jan. and Feb. 1884. By Capt. D. W. Barker.

(*Communicated by Capt. H. Toynbee.*)

Date.	Lat. S.	Long. W.	Bar.	Alt. Ther.	Air Ther.	Corr. Chron. Time G.M.T.	* from which Dist. of Comet's Nucleus was measured.	Dist. of Nucleus from star.	Remarks.
1884.	° ′	° ′				d h m s		° ′ ″	
Jan. 15	26 27	127 40	30·173	82	77	15 17 1 32	Achernar	60 27 22	Bright, well defined nucleus, with short and faint tail pointing in a N. direction; about $2\frac{1}{2}^{\circ}$ long.
						3 52	α Tauri	78 58 47	
						5 25	β Ceti	24 21 2	
16	27 43	126 9	30·217	83	77	16 16 44 28	Achernar	58 4 17	Tail faint; nucleus round, bright in centre, and envelope visible all round.
						46 29	α Tauri	78 5 47	
						47 48	β Ceti	22 4 7	
28	48 48	97 33	29·800	59	49·4	28 15 53 27	Achernar	36 31 39	Intrinsic brightness of nucleus about the same as α Sculptoris
						15 58 10	Aldebaran	72 38 4	
						16 1 29	β Ceti	6 30 19	
						16 3 48	θ Eridani	36 11 44	
30	50 57	90 14	29·982	59	47	30 15 24 44	Achernar	33 49 14	
						27 9	Canopus	68 48 24	
						30 5	Rigel	68 44 9	
			Not visible again till Feb. 16 on account of cloudy weather.						
Feb. 16	37 45	36 58	30·045	64	58	16 11 0 39	Achernar	17 33 59	Much fainter, and tail not distinguishable at all to naked eye.
						3 33	θ Eridani	19 5 19	
						7 5	α Tauri	72 7 34	

After Feb. 16 it became too faint for sextant observation. The chronometer time is corrected for rate, and the distances for index error of sextant.

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Sextant Observations of Comet Pons-Brooks, made on board the ship "Earnock." By Capt. George F. Parson.

(*Communicated by Capt. H. Toynbee.*)

1884.	A.T.S.	G.M.T.	Lat. S.	Long. W.	Aldebaran.	β Ceti.	Sirius.	Achernar.	Fomalhaut.	Canopus.	α Arietis.	Rigel.	Markab.
	P.M.	d h m s	° ′	° ′	° ′	° ′	°	° ′	° ′	° ′	° ′	° ′	° ′
Jan. 15	8 10	15 10 28 41	12 15	32 35	78 59	24 39*							
16	8 33	16 10 55 30	14 48	33 10	78 15		106 39		26 36				
17	7 48	17 10 10 0	17 0	33 2	77 29				25 27	92 38		84 4	22 55†
	8 20	17 10 43 0				20 19			25 17				23 1†
18	7 50	18 10 12 0	19 6	33 6		18 10			24 9			82 20	24 8‡
19	8 15	19 10 40 0	21 50	33 0		16 13			23 9			80 49	25 29§
20	8 0	20 10 20 0	24 14	31 50		14 20			22 20			79 29	26 42§
21	8 24	21 10 38 0	25 50	30 32		12 31			21 39			78 7‖	
23	8 35	23 10 35 0	25 48	27 0		9 22		44 52	20 42		49 16	75 32¶	
25	8 15	25 10 15 0	29 35	25 56		7 1		41 33	20 30		50 23	73 19**	
26	8 50	26 10 40 0	31 0	24 10		6 18		39 59††					
27	9 5	27 10 47 0	32 40	22 0		6 4		38 19				71 16	
30	10 0	30 10 53 0	38 40	10 28				34 5				69 5‡‡	

Feb. Owing to moonlight nights or cloudy weather no observations were obtained until Feb. 22, when the comet (invisible to the naked eye) was seen by the aid of an ordinary ship's telescope about 1° or 1½° N.E. by E. from an unnamed star, whose distances are as under. The comet lay somewhat to the right of a line from said star to Canopus, bearing and distance being of course only estimated.

				R.								
22	8 35	22 4 4 0	41 30	70 50				14 8				60 17	
Mar. 3	8 0		42 0	113 0	Comet distinguishable through telescope, but too faint for sextant observations.								

* Nucleus about 2nd magnitude, tail N.E. by E. 4°.

† Nucleus not visible until after stars of 3rd and 4th magnitudes have appeared, though after dark it appears about as bright as Markab, though less clearly defined.

‡ Tail very indistinct, though it lights up when a cloud obscures the larger stars in its vicinity.

§ Good observations. Tail more distinct. ‖ Tail visible halfway to η Ceti. ¶ Tail brighter than usual.

** Good observations. †† Tail visible halfway to β Ceti. ‡‡ Observations not very good.

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Sextant Observations of Comet Pons-Brooks, January 1884. By Capt. A. S. Thomson.

(*Communicated by Capt. H. Toynbee.*)

Position. Lat. N.	Position. Long. W.	Date.	G.M.T.	Centre of Nucleus and Star	Angular distances.	Barom. at 32° F. and at sea level.	Temp. of Air, F.	Remarks.
° ′ ″	° ′ ″		h m s		° ′ ″	in.	°	
28 28 0	16 0 45	Jan. 17	7 57 40	Algenib	23 0 54 mean of 3 obs.	30·32	65	The tail of the comet which shines faintly appears to the naked eye about 4° of arc in length, pointing towards the Pleiades. The nucleus is ill-defined, and its apparent diameter is about equal to that of the planet Jupiter. The angular distances observed between two stars are noted as an indication as to the value of the comet distances observed under similar conditions.
			8 17 40	α Pegasi	21 20 51 „ 3 „			
			8 44 23	Algenib & α Pegasi	16 31 1 single obs.			
			8 48 23	β Ceti	20 34 49 „			
			9 7 54	Algenib	23 2 50 mean of 2 obs.			
28 26 30	16 5 0	18	8 19 30	Algenib	24 10 17 „ 3 „	30·32	67	
			8 35 10	α Pegasi	23 35 17 „ 3 „			
			8 47 10	β Ceti	18 28 23 „ 3 „			
			8 58 40	Algenib & α Pegasi	16 29 59 „ 3 „			
28 34 40	15 43 20	19	8 32 15	Algenib	25 24 59 „ 2 „	30·41	65	The tail of the comet now points a little to the north of the Pleiades.
			8 57 20	α Pegasi	26 33 44 „ 3 „			
			9 6 0	β Ceti	16 25 44 „ 3 „			
			9 15 0	Algenib	25 25 10 single obs.			
			9 30 0	Algenib & α Pegasi	16 30 41 „			

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Position. Lat. N.			Long. W.			Date.	G.M.T.			Centre of Nucleus and Star	Angular distances.				Barom. at 32° F. and at sea level.	Temp. of Air, F.	Remarks.
°	′	″	°	′	″		h	m	s		°	′	″		in.	°	
28	30	20	16	2	30	Jan. 20	8	24	18	Algenib	26	44	11	mean of 3 obs.	30·40	64	
							8	39	4	α Pegasi	28	33	50	„ 3 „			
							8	50	14	β Ceti	14	31	15	„ 3 „			
							9	2	56	Algenib & α Pegasi	16	30	41	single obs.			
28	28	0	16	14	0	21	8	53	54	Algenib	28	8	16	mean of 3 obs.	30·45	64	
							9	9	12	α Pegasi	30	34	46	„ 3 „			
							9	19	5	Algenib & α Pegasi	16	29	28	„ 3 „			
28	28	0	16	14	0	22	8	10	24	Algenib	29	31	33	„ 3 „	30·50	63	The tail of the comet is becoming gradually fainter; is about 4° of arc in length, and points towards a position midway between the Pleiades and Aldebaran.
							8	20	14	α Pegasi	32	27	16	„ 3 „			
							8	33	27	β Ceti	11	2	14	„ 3 „			
							8	52	24	Algenib & α Pegasi	16	30	31	„ 4 „			
28	28	0	16	0	0	23	8	58	3	Algenib	31	10	10	single obs.	30·47	62	
							9	4	3	β Ceti	9	28	26	„			
							9	7	53	Algenib	30	58	46	„			
28	28	0	16	5	0	24	8	39	52	β Ceti	8	12	12	„	30·45	63	The tail of the comet now appears about 5° of arc in length, and points to Aldebaran.
							8	51	52	Algenib	32	23	47				

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Position. Lat. N.	Position. Long. W.	Date.	G. M. T.	Centre of Nucleus and Star	Angular distances.	Barom. at 32° F. and at sea level.	Temp. of Air, F.	Remarks.
° ′ ″	° ′ ″		h m s		° ′ ″	in.	°	
28 24 0	16 9 0	Jan. 25	9 1 25	Algenib	33 48 36 single obs.	30·44	62	
			9 8 10	β Ceti	7 7 3 „			
28 26 0	16 6 0	26	8 12 54	Algenib	35 11 36 mean of 3 obs.	30·40	64	
			8 23 8	β Ceti	6 27 17 „ 3 „			
			8 32 54	Algenib	35 12 48 „ 3 „			
			8 40 21	β Ceti	6 26 32 „ 3 „			
28 26 40	16 5 50	28	8 8 34	Algenib	37 55 10 single obs.	30·38	66	The tail of the comet now appears very dimly, and points a little to the southward of Aldebaran.
			8 22 34	β Ceti	6 17 45 „			
			8 34 4	α Pegasi	42 43 16 „			
28 28 0	16 6 0	29	8 29 53	Algenib	39 14 28 „	30·33	66	
			8 37 48	β Ceti	6 47 45 „			
			8 42 8	Algenib	39 13 57 „			
30 31 0	15 56 15	30	8 27 28	α Pegasi	45 37 45 „	30·23	67	
			8 33 58	β Ceti	7 29 28 „			
28 29 0	16 10 0	31	8 36 37	β Ceti	8 24 59 „	30·29	67	The moonlight and low altitude of the comet now cause the nucleus to be too indistinct for further observation with the sextant.
			8 48 7	α Pegasi	47 1 43 „			

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Elements of the Orbit of Comet a, 1884. By J. Tebbutt.

My observations of Pons' Comet having been interrupted on several occasions by cloudy weather, I have been enabled to devote some time to the calculation of the orbit of the comet whose positions I sent to you by a former mail. For the determination and verification of the elements I selected the following four Windsor positions, which are given with reference to the ecliptic and the mean equinox of 1884·0, but are uncorrected for aberration or parallax.

Greenwich M.T.	λ	β
d	° ′ ″	° ′ ″
Jan. 18·98218	327 23 13·7	−31 51 4·0
22·97140	331 9 25·7	−33 42 43·7
27·95377	334 58 39·6	−35 21 57·5
Feb. 1·98249	338 10 39·7	−36 36 2·9

As the comet was very faint at the time of the last observation, I adopted the first three places as a foundation for the calculation, and arrived at the following system of parabolic elements:—

T	... ...	= 1883, Dec. 25·30038, G.M.T.
π	... ...	= 125° 44′ 23″·9 } Mean Equinox,
Ω	... ...	= 264 23 59 6 } 1884·0.
i	... ...	= 65 0 54·8
log q	... ...	= 9·4910464
q	... ...	= 0·3097750

Motion Retrograde.

These elements reproduce the first three positions thus, the residuals being regarded in the sense of *calculation minus observation*:—

	$\Delta\lambda \cos\beta$	$\Delta\beta$
Jan. 18	−0″·7	+0″·6
22	−3·9	+4·0
27	−0·2	0·0

My last observation is represented thus:—$\Delta\lambda \cos\beta = -14''{\cdot}0$, $\Delta\beta = +14''{\cdot}4$, so that the above set of elements may be regarded as fairly representing the Windsor observations uncorrected for aberration and parallax. I trust when more extended observations are available to be able to take these corrections into account. No notice of this comet has appeared in the *Astronomische Nachrichten* to January 8, the latest date to hand, so it

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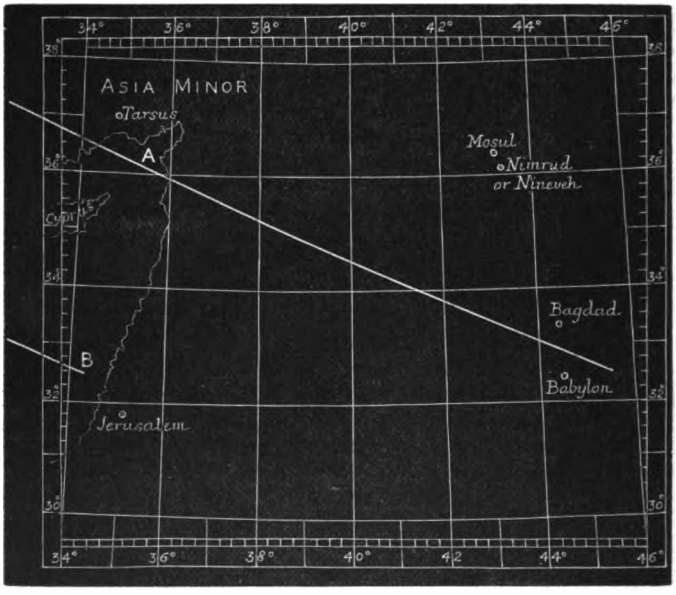
is highly probable that the comet has escaped observation in the northern hemisphere. I trust, therefore, that the observations and elements which I have communicated may be of interest to the Society.

Observatory, Windsor, N.S.Wales:
1884 *March* 4.

Note on the Eclipse of Thales. By J. Maguire.

The solar eclipse of Thales, B.C. May 28, 585, has been calculated by eminent astronomers from Hansen's Lunar Tables, altered and unaltered. At the time of their publication, more than a quarter of a century ago, they were used by Sir G. B. Airy, then Astronomer Royal, for the calculation of this eclipse in Asia



Minor, as shown on his map in vol. xxvi. of the *Memoirs*. In this volume the latitudes and longitudes are given for points on the central and limiting lines of the shadow, but not the times at which the phase occurs. I adopted the Right Ascensions and Declinations of the Sun and Moon at the hours of two and four, as given under the letter K in the elements; and though I had no doubt of their accuracy, I nevertheless extracted all the par-

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ticulars from the tables, and found a difference of only 3″ in the Moon's Right Ascension. For $3^h\ 37^m$ the data will be

		R.A.		Dec.	
☾	...	58 12 43·3		20 54 46·9	(D)
			a corr.	0 7.7	
☉	...	57 19 44·6		20 20 43·6	
a	...	0 52 58·7	*x* ...	0 34 11	

$$P - n = 3{\cdot}5643583$$
$$p\ 36^\circ\ \ 9{\cdot}9995010$$
$$P' \qquad 3{\cdot}5638593$$

The equation of time is taken $+9^m\ 8^s$, and the calculations made according to the method of Woolhouse, the resulting latitude being 35° 57′ 32″, and longitude 35° 50′ 45″: A representing this position on the accompanying map. Four more points on the central line have been calculated—the last being at $3^h\ 38^m\ 36^s$ in latitude 32° 28′ 50″, and longitude 45° 33′ 8″, the place of central ending—Bagdad and Babylon being within the limits of totality.

In 1872 it appears that Dr. Hind wrote a letter to the *Times*, which was copied into the *Astronomical Register* for September in that year, and giving an interesting account of several historical eclipses, among which is the famous one of Thales. He states at the outset that he employed the last value of the secular acceleration of the Moon's mean motion given by Hansen, and combined other important elements determined by him with the result of Leverrier's Tables of the Sun. He says his new calculation indicates that the eclipse was *total* in Nimrud for between three and four minutes shortly before sunset.

In 1879 Professor S. Newcomb, from his own Tables has placed the track of the *central* eclipse in the Mediterranean, the last position given being at B on the map, in latitude 32° 30′, and longitude 34° 12′.

As the celebrated and decisive battle between the Medes and Lydians is not supposed to have been fought near Jerusalem, nor to have been a naval engagement in the Mediterranean, the Professor's determination shows that if the battle-field is assumed to be in Asia Minor, the eclipse there must have been a *partial one*.

These are rather discordant results, the difference between the north and south determinations of centrality being about six degrees.

Norwich: 1884 *May* 13.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLIV. JUNE 13, 1884. No. 8.

EDWIN DUNKIN, Esq., F.R.S., President, in the Chair.

Robert Stuart Callcott, 1 Bath Place, Kensington, W.; and Samuel Johnson, M.B. (Edin.), 23 Gloucester Terrace, Hyde Park, W.;

were balloted for and duly elected Fellows of the Society.

Notes on Mr. Stone's Explanation of the Errors of Hansen's Lunar Tables. By Professor Simon Newcomb.

The courtesy afforded me by the Royal Astronomical Society in publishing my questions respecting Mr. Stone's theory, which appeared in the March number of its *Notices*, leads me to enter a little more fully into the case, and to show the very important bearing which those questions have upon the applicability of the theory.

Mr. Stone is understood to maintain that no one has answered his arguments. To answer an argument one must first understand it; and I frankly confess that this is more than I have been able to do. In particular I have been entirely unable to master the reasonings and equations found on p. 336 of vol. xliii. and p. 226, vol. xliv. But the value of an argument depends largely upon its conclusions; and it is the defect in this respect which I wish to point out.

So far as Hansen's Tables are concerned Mr. Stone has not even attempted to make out a logical case. This will be made clear by showing what his conclusion is, and how alone it can be proved. I understand the conclusion to be—

"That the increasing errors in Hansen's Tables of the Moon since 1864 arise from the substitution of Le Verrier's Solar Tables for Carlini's in the British *Nautical Almanac* for 1864 and subsequent years."

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Now the logical process for establishing this proposition are clear and simple, and may be stated as follows:—

A. Discover what the apparent errors of the Lunar Tables would have been had Carlini's Solar Tables been continued in use until the present time.
B. Show what the errors actually are under the influence of Le Verrier's Tables. The difference of these results will show the effect of the change of tables.
C. Determine what the theoretical errors are arising from the cause Mr. Stone has discovered.

The proof would then consist in showing that C corresponded to B minus A.

Now, Mr. Stone has made no attempt to cover more than one of these three points. He gives B, the actual errors of the Lunar Tables, very fully, on p. 337 of vol. xliii. But he does not profess to have made any calculation showing what these errors would have been had Carlini's Tables been continued. This is the first logical defect. Secondly, he does not state, except in algebraic form and with an unknown symbol t, what the difference would be according to his theory.

Thus of the two terms of comparison B—A and C, whose agreement is necessary to the *prima facie* establishment of his theory, he gives neither.

Not only has Mr. Stone suffered these two fatal defects in his argument to stand a year, but he has resisted all my appeals to give a general definition of his symbol t, which shall apply to any ephemeris whatever, and thus enable anyone to calculate the result of his theory.

Now, were I to attempt to apply Mr. Stone's theory in what would seem the most natural way, I should count the time t from the epoch 1728, when the two tables agreed in mean longitude. This would make a deviation at the present time of nearly two minutes, whereas the actual errors of the tables are scarcely more than one-tenth of this quantity. If this interpretation is correct the theory could be established only by showing that the errors of the Lunar Tables are two minutes different from what they would have been had not the change of solar tables been made.

Apparently, though Mr. Stone nowhere explicitly says so, he desires the computer to count the epoch t from the year 1864, when the new tables were introduced into the British *Nautical Almanac*. Now, a large number of observations of the Moon have been compared with the data of the American Ephemeris, in which Hansen's Solar Tables, the mean motion of which is substantially identical with those of Le Verrier, were introduced in 1858. If I adopt the epoch 1858 the result will be a correction different by $4''.5$ from that applicable on the same theory to the British *Nautical Almanac*. But, as a matter of fact, all the

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numerical data of the two ephemerides are substantially identical; so that the formula must be the same for both. If, on the other hand, I take the year 1864 as the epoch for the American Ephemeris, I proceed on the absurd theory that the results of observations and computations made in a country 3,000 miles away are now several seconds different from what they would have been had Le Verrier's Tables never been introduced into the British *Nautical Almanac*.

In a word, to count from any other epoch than 1728 involves the absurdity that the result of computing from a set of tables is a function of the time when some particular person began to use those tables. I have found this difficulty insurmountable. I think Mr. Stone will also find it insurmountable. Indeed, his refusal to make known so simple a matter as what the deviations of the lunar tables in the American Ephemeris ought to be on his theory seems to me tantamount to an admission that his theory cannot be applied to observations and computations made outside of England.

Addendum.

The following statement of the dilemma in attempting to apply Mr. Stone's theory may be more clear and explicit:—A comparison of Hansen's Lunar Tables with observation by means of the data of the American Ephemeris shows the same phenomena as in the comparison by means of the British *Nautical Almanac*—namely, a close agreement up to the year 1863, and from that epoch a steadily increasing deviation. The numbers are in fact substantially the same for both ephemerides. Hence the corrections must be the same for both. But if I adopt Mr. Stone's theory, and apply the same corrections, I implicitly assume that this deviation, which dates from 1863 in our ephemeris, is due to the adoption of new tables in another country. I do not see how to apply the theory without this absurdity.

Answers to Prof. Newcomb's Questions on the Changes in the Adopted Unit of Mean Time. By E. J. Stone, M.A., F.R.S.

I greatly regret to find that my views have not been explained with sufficient clearness to enable Prof. Newcomb to deal directly with my arguments. All I want is to have a most serious question rightly settled. If my views are right a new departure can be made in astronomy, and a considerable advance would be certain to follow. If my views are wrong it is most desirable that they should be so completely disproved that their revival at some future period may be effectually prevented, and that our attention should be directed to some other physical explanation of our difficulties.

That our estimations of the length of the tropical year

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change *per saltum*, and that our Lunar Tables require complete readjustment to an exactly proportionate amount from the adoption of new Solar Tables, are facts whose connection and significance cannot safely be overlooked.

The agreement between Hansen's Tables and observation when the mean times are reduced to one uniform scale, in accordance with my views, is much closer, as a matter of fact, than that produced by the empirical corrections to Hansen's Tables introduced by Prof. Newcomb, although these corrections are based upon a discussion of recent observations with the introduction of at least four disposable constants to secure an agreement between the corrected tabular places and observation.

I have not the slightest difficulty in meeting Prof. Newcomb's dilemma in the most direct manner; but my way of meeting it will not be satisfactory to Prof. Newcomb unless he can realise the possibility of what I am quite sure is a fact—that we are not employing in our practical astronomy the true mean solar day as our unit of time. To assume that such is the case without proof is simply to assume the whole matter at issue. That the same angle can be represented by either $(360°+n)t$ or $(360°+n+\delta n)t$ is simply impossible; and that the angles $360°t$ and nt can be separated, tabulated on different scales, and the results afterwards combined without error, is equally impossible. That the errors introduced by such suppositions can be neglected in the present state of astronomy is clearly a matter of proof and not of assumption.

But even if Prof. Newcomb does believe, as astronomers have hitherto believed, that we do use the true mean solar day as our unit of time in our practical astronomy, I beg of him, from the importance of the subject, to simply follow my ideas to their logical consequences, and to examine whether there are any facts inconsistent with them, and whether the results deducible from them do not afford explanations of discordances which have hitherto been otherwise inexplicable. It is only in this way that advances in science can be made.

Let

$$(1)\quad \text{R.A. of Greenwich meridian} = s = a + \left(\frac{n}{15} + 24^h\right)t + \sigma t^2 + \text{nutation}.$$

The epoch being 1800 Jan. 1, G.M.T., and t reckoned in units T, or so-called mean solar days whose length has hitherto been supposed to be given by the equation

$$\text{T} = \text{sidereal day}\left(\frac{360° + n}{360°}\right),$$

when A and n are Bessel's adopted constants we know, as a matter of fact, that Hansen's Lunar Tables represent the observations very closely from 1750 to 1864, when we determine our time t from (1) in the usual manner. If a careful examination of our method of determining mean time is made,

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or of the method of determining the R.A. and N.P.D. on the meridian, for comparison with our observations, from the tabular places for assumed Greenwich mean times, which are directly given by our Lunar or Solar Tables, it will be seen that we start each day from the assumed R.A. of the meridian at "mean noon" as a *fixed point*. All the rest of the calculation is merely differential, and any resulting errors are small from the rapid rotation of the Earth upon its axis. If, therefore, we do change our unit of time we must so change it that our tabular places may correspond still to the true sidereal time at the adopted mean noon used in the construction of the tables for the determination, on the scale of time T, of the tabular mean motions. It is from the want of a clear perception of the importance of this point that the present discussion has been possible. If this condition was not necessary we could change the unit of time by putting

$$t = \left(1 + \frac{\delta n}{360^\circ + n}\right)t',$$

as is generally assumed to be the case, and then determine t' from s from the equation

$$s = \alpha + \left(24^h + \frac{n + \delta n}{15}\right)t' + \text{etc.} \ldots$$

But taking $t' = r$ an integer, we deduce from this expression

$$sn = \alpha + (n + \delta n)r,$$

and not nr, which is its value at the *true* tabular mean noon of our adopted scale.

If, therefore, we will change our unit of time, and want to still tabulate the sidereal times at mean noon and to avoid errors, we must change the units so that nt and $360^\circ t$ are proportionately changed. If we put

$$nt = (n + \delta n)t',$$

then

$$s = \alpha + \left\{\frac{(n + \delta n)}{15}t' + 24^h\left(1 + \frac{\delta n}{n}\right)t'\right\} + \text{etc.};$$

and we have, when $t = r = r^{\text{th}}$ noon of original scale, to which our Lunar Tables correspond,

$$sn = \alpha + nr + \text{etc.},$$

which is the true value. But if I use, instead of the expression

$$s = \alpha + \left(\frac{n + \delta n}{15}\right)t' + 24^h.t' + 24^h.\frac{\delta n}{n}.t' + \sigma\left(1 + \frac{\delta n}{n}\right).t'^2 + \text{nutation},$$

the expression

$$s = \alpha + \left(\frac{n + \delta n}{15}\right)t' + 24^h.t' + \sigma t'^2 + \text{nutation},$$

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the first gives me the correct, the second the erroneous result, when t' is put equal any integer r, and the result used as the sidereal time corresponding to that of the r^{th} noon from the original epoch. I think I am fairly entitled, therefore, to ask why I must use the erroneous result, and how the tabular quantities can be expected to agree with observations when I thus clearly make a mathematical error, and how Hansen's Tables are supposed to adapt themselves to changes made in 1858 or 1864, after their construction? The whole question, therefore, turns upon this point: Can I, so long as I determine my mean times as at present, neglect the term

$$24^h.\frac{\delta n}{n}.t'$$

in the expression for s when the term nt has been changed into

$$(n+\delta n)t'?$$

If I can it must be proved, not assumed; and no proof has as yet been attempted. Directly I introduce the correction required to secure continuity in our measures of time, viz.

$$24^h.\frac{\delta n}{n}.t'$$

into the expression for the R.A. of the meridian, all Prof. Newcomb's difficulties disappear.

For if the American and English astronomers are simply neglecting the same term

$$24^h\frac{\delta n}{n}.t'$$

in their computations of the so-called sidereal times at mean noon, then their calculated sidereal times at mean noon will agree and be equally in error; and therefore their deduced mean times will be equally in error; and these equal errors in mean time will cause the tabular places computed for erroneous times to be equally in error, and the discordances between the tabular places and the Greenwich and American observations will, therefore, be equally large after the year 1864. There will be differences between 1858 and 1863, but these differences will be small, and will be considerably disguised by the fact that Hansen's longitudes were too small during the years 1858–1863, as shown by the Greenwich observations; and the effect of the change during these years will be simply to make the tabular places agree rather more closely with the observations.

In the following table I give the excess of Hansen's longitude over observation for the Greenwich observations for some years about the critical period 1858–1864. The value of (H−O) in longitude from the Greenwich observations was $+1''$ in 1847, and then continued about $-1''$ until 1864. The excess now is over $14''$.

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The Greenwich observations for the years 1859, 1860, and 1861 have not yet been compared with Hansen's Tables, but this is now to be done by the Society. Supposing, however, as Prof. Newcomb states, that the Washington observations agree closely with the Greenwich observations, I have given some residual errors on the supposition that the Washington observations give identical results with those derived from the Greenwich observations. It will be seen that no reliable evidence either for or against my views can be thus obtained.

Year.	Error due to Change in Scale of Time between Washington and Greenwich.	Observed Value of H−O from Greenwich Observations (Longitude).	Washington Residuals corrected for Error in Time on the supposition that the Uncorrected Residuals are identical with those obtained from the Greenwich Observations.
	″	″	″
1855	0·0	−0·9	−0·9
1856	0·0	−1·0	−1·0
1857	0·0	−1·9	−1·9
1858	+0·4	−2·0	−1·6
1859	+1·1	$-x$	$+1{\cdot}1-x$
1860	+1·9	$-y$	$+1{\cdot}9-y$
1861	+2·6	$-z$	$+2{\cdot}6-z$
1862	+3·3	−2·8	+0·5
1863	+4·1	−1·6	+2·5
1864	0·0	+0·1	+0·1
1865	0·0	+1·2	+1·2
1866	0·0	+2·1	+2·1
1867	0·0	+3·5	+3·5

It is impossible to fix empirically within several years the exact period at which a regular increase of 0″·7 per annum commences, when the periodic fluctuations amount to several seconds. For instance, there is actually a decrease of H−O between the years 1875 and 1878, due to periodic and other errors; and therefore I lay no stress whatever upon the fact, but the application of these corrections would sensibly make the Washington observations appear to run away from about 1863, and Prof. Newcomb asserts that such is actually the case. The Washington lunar observations are, however, unfortunately not very numerous or well distributed over every lunation.

The correction in the years 1859, 1860, 1861 would therefore simply tend to produce a somewhat better agreement between the tabular places and the observed. If I had attempted to determine the period at which the regular increase commenced empirically I should not have fixed on the year 1864, but 1862, and should then have had to compute the residuals for 1859, 1860, 1861, before attempting to decide such a point empirically. For it is quite possible that Hansen's Tables might be in defect

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instead of excess in 1860 or 1861 to a greater extent than in 1864. It was not, however, until after I had discovered the cause of the running away and found its amount approximately from an examination of the solar theory that I had these mean errors, or discordances, between observations and Hansen's Tables collected. There is, therefore, not the slightest force in Prof. Newcomb's dilemma unless he can prove, and not assume, that the term

$$24^h . \frac{\delta n}{n} . t'$$

is not required as a correction to

$$s = a + \left(\frac{n}{15} + 24^h\right) t'$$

to reduce our mean times to the scale of those adopted before 1864 in the British and before 1858 in the American *Nautical Almanacs*, when we determine our mean times from the adopted sidereal times at mean noon and refer our Right Ascensions of the stars used for clock error to mean times through the tropical year as at present. t' is here reckoned from the original epoch, 1800 Jan. 1, G.M.N.

With reference to Prof. Newcomb's statements A, B, and C, I have given not only the discordances between Hansen's Tables and the Greenwich observations when the times are uncorrected, but also the errors when the times are reduced to the standard of Carlini's Tables by the application of the proper correction,

$$24^h . \frac{\delta n}{n} . t',$$

for the Greenwich observations from 1864 to 1881, and for the Radcliffe observations during the years 1882 and 1883. All these results accord with the correctness of my views. The necessity for this correction dates from 1864 Jan. 1, G.M.N., for the Greenwich and Radcliffe observations. This is my answer to A and B. Of course the correction

$$24^h . \frac{\delta n}{n} . t'$$

must only be applied when the mean times actually adopted for the reduction of the observations have been computed with the value $n + \delta n$ instead of the value n; and it would only be whilst the times were thus computed for successive years that an agreement between Hansen's Tables and observations could be made by the method I pointed out in the *Monthly Notices* (vol. xliii. No. 7) for May 1883 by applying a decrease of $0''.747$ per annum to the mean longitudes of Hansen's Tables, the decrease being of course reckoned from the time the error first commenced till the time the error ceases to be made; and in the observations I was discussing this change was, as I clearly stated, made in 1864,

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and I have actually drawn a line between the results for 1863 and 1864 in the tabular matter to show the break of continuity (see p. 337). I am, therefore, at a loss to understand Prof. Newcomb's doubt about the meaning of my *t*.

At present I have not attempted to correct Hansen's Tables at all, or touched their results in any way whatever, but merely presented the real errors of Hansen's Tables. This is my answer to question C. Prof. Newcomb's evident misconceptions about this matter are due simply to his not realising the fundamental character of the question. Two independent things are required before tabular places can be compared with observations —viz. the tabular places computed directly from the Tables, and the *mean times from observation* for which the tabular places are to be interpolated or directly computed. If your tables are wrong the results will disagree from observation; but if *you will change the scale upon which the time is measured, and not allow for the differences between your times and those adopted for the construction of the tables*, then the resulting discordances are not due to the tables at all, but to your work, *and these errors will date not from the epoch of the tables, but from the time you commenced to make those changes*. There is one point to which attention should be called. If n is Bessel's adopted value of the Sun's mean motion, and ω the angular velocity of the Earth about its axis, we have

$$\frac{\text{Tropical year}}{\text{Sidereal day}} = \frac{\omega}{n}.$$

If, therefore, we consider, as we have hitherto done, that ω is an absolute constant—a supposition which cannot be true if our unit of time is changed in the slightest degree—but that n can vary, we must have different lengths of the tropical year with every change of n, and the result to which this leads is as follows.

The tropical year for a long period to 1858 contains some definite number of sidereal seconds. In 1858 we had two tropical years differing $1^s{\cdot}46$, according as we took up the English or American *Nautical Almanac*. In 1864 the English changed their tropical year. Now I would ask, What is the meaning of such changes in estimation? They must be due to errors in our methods of measuring time; and the magnitude of the errors thus indicated is *exactly* that which is shown by the discordances between theory and observation in the case of the lunar observations, and confirmed, although of course only slightly shown, by the solar observations also. If my views are accepted there is no change of the tropical year at all. All I assume is that nothing of a physical character has changed.

The comparatively large change of unit which took place in 1864 is, however, mainly due to the artificial manner in which our mean times are computed. This method has been based on the assumption that we were, in all cases, using the true mean solar day as our unit; and we have attempted to keep the

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principal term in s, $24^h \times t$, unchanged, however much we may have changed n, and therefore our unit of time, and have thus thrown the adjustment either upon the nt or upon the a, which has thus become a variable instead of a constant.

If we redetermined all our mean times, starting with the constants now adopted in Le Verrier's Solar Tables, and therefore no longer referring our tabular places to the mean noons on Bessel's scale, then the adopted unit of time would only appear to contain about $24^h\ 3^m\ 56^s{\cdot}555144$.

The effects of the term

$$24^h . \frac{\delta n}{n} . t'$$

would then be included in the new determinations of the constants of mean motion of the Sun, Moon, and planets, which would or should refer the tabular places to the new mean noons. In the present case I have treated the question entirely upon the supposition that the n and $n+\delta n$ are mere adopted angles, and assume no physical connection with the Sun's mean motion at all directly, but merely continuity in our time measures. Indirectly, however, the assumption is tacitly made in our practical work by the way in which our Right Ascensions are referred to epoch through the tropical year. I am, however, led to exactly the same results as when I discuss the question under the view that n and $n+\delta n$ both represent the Sun's real motion in longitude. In the latter case, however, the true unit of time T is seen at once to be given by an equation of condition connecting the true mean solar day T_o, the sidereal day s, T, and n; for

$$\omega = n + 360^\circ . \frac{T}{T_o},$$

an equation which I have proved from first principles, and which must be proved to be erroneous before I can be shown to be wrong, since if it be true we have attempted to carry on our approximations by retaining a term δn and neglecting the term

$$360^\circ \frac{\delta n}{n},$$

which is 365·25 times as large. Under this view, if we assume that we are using true mean solar times, my results simply give the differences due to the errors of this assumption in true mean solar time arising from the change of units.

If Prof. Newcomb means by A that I ought to assume that you can now recover the mean times of Carlini's scale by merely replacing Le Verrier's sidereal times at mean noons by Bessel's, with the erroneous assumption that these mean noons will thus refer to the same instants of absolute time as they would have done had Carlini's Solar Tables been continued in use since 1864, and this although *Bessel's tropical year, which differs* $1^s{\cdot}46$ *from that adopted since* 1864, *has been replaced by Le*

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Verrier's to connect the sidereal times and mean times, then all I can say is, that I have done nothing, in my opinion, so absurd. To do this would simply be to assume that no change in our adopted unit of time did take place, although I have proved that in our computations of time we did replace the unit formerly in use by one which contained $0^s{\cdot}004$ per diem more, or $1^s{\cdot}46$ per year more. If I neglected this change I should simply get an erroneous result. The same thing has been proved by Prof. Cayley, who admits a change of unit equal to the 365th part of the change which I indicate; but who, in order to keep this change small, has to admit that he starts in 1864 *an independent mean Sun.* In such a case, of course, all our measures of time would be discontinuous after the change with those in use before the change; and to connect the two systems corrections would be required to be applied. And these corrections and his change of unit are simply those which are obtained by me combined by so changing the unit of time that no discontinuity is introduced. It appears to me that some astronomers are conducting this discussion as if we could have, compatible with accurate results, two independent and *inconsistent* measures of time—sidereal time and mean solar time. It will be found that I cannot be proved to be wrong by *simply assuming results which cannot be proved to be true except upon the assumption that we are using the true mean solar as our unit of time*, and then pointing out the fact that from these results there can be deduced consequences inconsistent with my views. I assert that we cannot, so long as we are changing from time to time our adopted values of the Sun's mean motion in longitude, measure our times from the motion of the meridian in Right Ascension in terms of the true mean solar day. And I have pointed out inconsistency after inconsistency to which the assumed truth of this has led us. But what I assume is at least a possibility, a thing within our power. I merely assume that, instead of using the time of the rotation of the Earth about its axis, or the "true sidereal day," as defined strictly by the time in which the Right Ascension of the meridian, cleared from the secular term and from the periodical terms of the nutation in R.A., increases 360°, we have preferred, as a matter of convenience, to adopt as our unit some number of sidereal seconds, which we believe will be found not to differ much from the length of the true mean solar day, whenever that interval of time shall have been accurately determined. Such a unit of time, once adopted, cannot change unless we change it. And if we change it we must allow in the usual way for the change, or we cannot escape the consequences of our neglect. The effects of changes when the unit is thus fixed are easily investigated, but the amount of the change will depend upon the way we use any adopted expression for the R.A. of the meridian

$$= \left(24^h + \frac{n}{15}\right) t$$

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to find t. If we assume that $24^h.t$ is independent of changes of nt, we assume that which cannot be true. If we use the expression after the change as if $24^h.t$ was independent of changes in nt, we simply replace by supposition the suppressed factor

$$\frac{T}{T_0}$$

of my formula

$$s = a + \left(24^h \frac{T}{T_0} + \frac{n}{15}\right)t',$$

and thus really find, instead of time on scale t after the change of n to $n+\delta n$, time on scale t_1, such that

$$nt = (n+\delta n)t'.$$

I have not thought it desirable to formally discuss Prof. Newcomb's statements about the epoch 1728, since they are obviously due to some serious misconception of the question at issue.

If Prof. Newcomb will refer to p. 266 of his "Researches on the Motion of the Moon," where he has given the numbers which must be subtracted from the times indicated by astronomical observations in order to reduce them to a *uniform measure of time*, viz. for

	s		s
1625	+12	1775	−15
1650	+35	1800	−17
1675	+34	1825	− 8
1700	+25	1850	0
1725	+16	1875	+14
1750	0		

whilst for −687 the error is supposed to be -70^m; and if he will ponder on his own remark: "These corrections are so minute that their independent detection by existing observations is barely possible," he will perhaps realise that it is no more possible to recover *absolute epochs* after changes in the data, unless the *principle* upon which such changes are to be allowed for is settled, than it would be to detect real changes in the absolute times of the Earth's rotation, supposing such were really taking place to any sensible extent.

The Right Ascension of the meridian is definite enough; but we have in our astronomical work to connect that definite direction with some mean time t. If we introduce changes into our determinations of t, and continue these for twenty years, it is simply impossible for us to recover the exact time which corresponds to a given Right Ascension of the meridian unless we can connect the present system of mean time measures with those in use in the past without any *breaks of continuity*; and if we had two independent systems of time, sidereal time and mean solar time, it would be impossible to pass from the one to the other, after breaks of continuity had been introduced, until the proper corrections to remove these breaks of continuity had been applied. I assert that I have given the mean times which

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would have been in use had Carlini's Tables been continued unaltered to the present time, and I have shown that when these necessary corrections are applied the discordances between theory and observation are sensibly removed.

I have given four independent proofs of the correctness of my views.

1. I have proved from first principles that

$$\omega = n + 360^\circ \frac{T}{T_0},$$

so that when we adopt a numerical value n for the Sun's mean motion in longitude, we do adopt a unit of time T in our practical astronomy just as much as when we adopt 360° for ω we adopt the sidereal day as our unit. The above equation is exact, and our expressions for sidereal times at mean noon are also exact; in fact, our time measures are exact, but expressed in terms of our unit T, whose ratio to the true mean solar day will only be exactly known when the ratio of the true mean solar day to the sidereal day is known, or when our solar theory is practically perfect.

Are these proofs of mine sound or unsound? If they are sound, then I must be right, since we have hitherto neglected the term

$$360^\circ \frac{\delta n}{n}$$

in the value of

$$\delta\omega = \delta n + 360^\circ \frac{\delta n}{n}$$

in carrying out our approximations, and have neglected the effects of the change of unit with every change of n.

The following are direct observational tests:—

2. In the following Table I show that the Right Ascensions of the stars do run away from their mean time epoch to the extent indicated by my views.

The Right Ascensions of the stars, by which our clock errors are determined, for the beginning of each year refer to the following mean times according as we employ Bessel's or Le Verrier's data: and this is the only relationship *directly established by observation between the sidereal times and mean times.*

Mean time of Sun's mean longitude being equal to 280° according to Bessel's or Le Verrier's scale of mean times.

	Bessel.		*Le Verrier.*		*LV.−B.*	
	d h m s		d h m s		m s	
1860	Dec. 31 15 13 11		Dec. 31 15 9 56		−3 15	
1865	30 20 17 8		30 20 13 46		−3 22	7
1870	31 1 21 5		31 1 17 36		−3 29	7
1875	31 6 25 2		31 6 21 26		−3 36	7
1880	31 11 28 59		31 11 25 15		−3 44	8
1885	30 16 32 55		30 16 29 4		−3 51	7

or change in 20 years = 29ˢ.

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It will be seen, therefore, that instead of the change in mean time due to the change from Bessel's to Le Verrier's data being measured by the mere numerical difference of the sidereal times at mean noon according to Bessel and Le Verrier on the false assumption that no allowance is required for the change of the position of the meridian between the two mean noons, these changes are 366 times greater. In fact, the adjustment has to be made by the Sun and not by the meridian, as has hitherto been assumed. It is clear, therefore, that to find the time on *Bessel's scale* when using Le Verrier's times, as we have done in England since 1864, we must apply the corrections indicated by my theory, or our tabular places are not correctly referred to the meridian.

3. Le Verrier's Solar Tables are sufficiently good to put in evidence the same change of unit; I have shown this by a discussion of the Greenwich observations since 1864.

4. Hansen's Lunar Tables indicate most *distinctly* a similar running away to exactly the same extent; and, on account of the rapid motion of the Moon the discordance between observation and theory uncorrected for the change of mean time already amounts to more than 14″, a quantity sufficiently large to be safely separated from personal and instrumental defects.

The Apparent Orbit of a Satellite of a Superior Planet. By J. Morrison, M.D., M.A., Ph.D., Assistant on the American Ephemeris, Washington, D C.

The apparent orbit of the satellite of a superior planet is the projection of the real orbit on a plane perpendicular to the line joining the Earth and planet, and is, therefore, in general an ellipse. In this paper I shall assume that the elements of the orbit of the satellite are known, and proceed to develop as much of the subject as is necessary for the computation of an ephemeris.

Let

α, δ, Δ be the polar coordinates of the planet as seen from the Earth;

α', δ', Δ' the polar coordinates of the satellite as seen from the Earth;

a, d, r the polar coordinates of the satellite as seen from the planet,

referred to the equator and equinox of the epoch for which the ephemeris is to be computed.

The general expressions for the rectangular coordinates in terms of polar coordinates are

$$x = \Delta \cos \delta \cos \alpha$$

$$y = \Delta \cos \delta \sin \alpha$$

$$z = \Delta \sin \delta.$$

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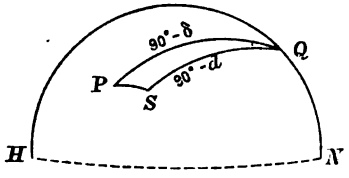
Therefore by equating the rectangular coordinates we have

$$\begin{aligned} \Delta' \cos \delta' \cos \alpha' &= \Delta \cos \delta \cos \alpha + r \cos d \cos a \\ \Delta' \cos \delta' \sin \alpha' &= \Delta \cos \delta \sin \alpha + r \cos d \sin a \quad \ldots \quad (1) \\ \Delta' \sin \delta' &= \Delta \sin \delta + r \sin d. \end{aligned}$$

Multiplying the first of this group by cos α, the second by sin α, and adding the results, then multiplying the first by sin α, the second by cos α, and subtracting, we have

$$\begin{aligned} \Delta' \cos \delta' \cos (\alpha' - \alpha) &= \Delta \cos \delta + r \cos d \cos (a - \alpha) \\ \Delta' \cos \delta' \sin (\alpha' - \alpha) &= r \cos d \sin (a - \alpha) \quad \ldots \quad \ldots \quad (2) \\ \Delta' \sin \delta' &= \Delta \sin \delta + r \sin d. \end{aligned}$$

We have now to consider the spherical triangle formed by the north pole of the equator, the planet, and the satellite; thus let Q be the north pole, P the planet, and S the satellite. In the spherical triangle Q P S we have



$$PQ = 90° - \delta, \; QS = 90° - \delta';$$

and

$$PQS = \alpha' - \alpha.$$

Put

PS = s, the apparent angular distance of the satellite from the planet as seen from the Earth, and

QPS = p, the angle of position of the satellite reckoned from the north toward the east and from 0° to 360°.

By the fundamental formulæ of spherical trigonometry we have

$$\begin{aligned} \sin s \sin p &= \cos \delta' \sin (\alpha' - \alpha) \\ \sin s \cos p &= \cos \delta \sin \delta' - \sin \delta \cos \delta' \cos (\alpha' - \alpha) \quad \ldots \quad (3) \\ \cos s &= \sin \delta \sin \delta' + \cos \delta \cos \delta' \cos (\alpha' - \alpha) \end{aligned}$$

Multiplying by Δ' and substituting from (2) this group becomes

$$\begin{aligned} \Delta' \sin s \sin p &= r \cos d \sin (a - \alpha) \\ \Delta' \sin s \cos p &= r \{\cos \delta \sin d - \sin \delta \cos d \cos (a - \alpha)\} \quad \ldots \quad (4) \\ \Delta' \cos s &= \Delta + r \{\sin \delta \sin d + \cos \delta \cos d \cos (a - \alpha)\}, \end{aligned}$$

from which we must eliminate a, d, and Δ'.

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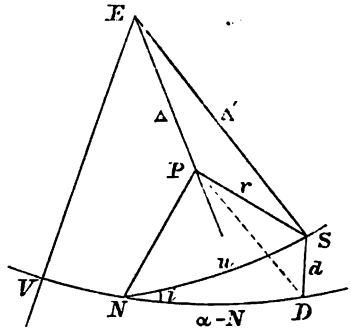
Let V D represent an arc of the equator, N S an arc of the orbit of the satellite, E the Earth, P the planet, and S the satellite; draw the arc S D perpendicular to V D, and let E V be the direction of the vernal equinox. Then N will be the



position of the ascending node of the orbit of the satellite on the equator; V D and S D the planetocentric Right Ascension and Declination of the satellite, which we have denoted by α and d respectively.

Put

VN = N, the right ascension of the ascending node;

SND = i, the inclination of the orbit to the plane of the equator;

NS = u, the angular distance of the satellite from the ascending node;

then

ND = $\alpha - N$.

From the right-angled spherical triangle N S D, whose centre is P, we have

$$\begin{aligned} \cos d \cos(\alpha - N) &= \cos u \\ \cos d \sin(\alpha - N) &= \sin u \cos i \quad \ldots \quad \ldots \quad \ldots \quad (5) \\ \sin d &= \sin u \sin i. \end{aligned}$$

Multiplying the first of this group by cos $(\alpha - N)$, the second by sin $(\alpha - N)$, and adding the results; also multiplying the first by sin $(\alpha - N)$ and the second by cos $(\alpha - N)$, and subtracting, we have

$$\begin{aligned} \cos d \cos(\alpha - \alpha) &= \sin u \cos i \sin(\alpha - N) + \cos u \cos(\alpha - N) \\ \cos d \sin(\alpha - \alpha) &= \sin u \cos i \cos(\alpha - N) - \cos u \sin(\alpha - N) \\ \sin d &= \sin u \sin i, \end{aligned}$$

which substituted in (4) give, after simple reductions—

$$\begin{aligned} \Delta' \sin s \sin p &= r \{\sin u \cos i \cos(\alpha - N) - \cos u \sin(\alpha - N)\} \\ \Delta' \sin s \cos p &= r \{\sin u [\cos \delta \sin i - \sin \delta \cos i \sin(\alpha - N)] \\ &\qquad - \sin \delta \cos u \cos(\alpha - N)\} \\ \Delta' \cos s &= \Delta + r \{\sin u [\sin \delta \sin i + \cos \delta \cos i \sin(\alpha - N)] \\ &\qquad + \cos \delta \cos u \cos(\alpha - N)\}, \end{aligned}$$

from which Δ' must be eliminated.

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In the plane triangle E P S formed by joining the Earth, planet, and satellite, let σ denote the supplement of the angle E P S; then from the diagram (fig. 2) we have

$$\Delta' \sin s = r \sin \sigma$$

and

$$\Delta' \cos s = r \cos \sigma + \Delta \quad \ldots \quad \ldots \quad \ldots \quad (6)$$

which being substituted in the equations of the last group, give after reduction

$$\begin{aligned} \sin \sigma \quad \text{n } p &= \sin u \cos i \cos (\alpha - \text{N}) - \cos u \sin (\alpha - \text{N}) \\ \sin \sigma \cos p &= \sin u \,[\cos \delta \sin i - \sin \delta \cos i \sin (\alpha - \text{N})] \\ &\qquad - \cos u \sin \delta \cos (\alpha - \text{N}) \quad \ldots \quad (7) \\ \cos \sigma &= \sin u \,[\sin \delta \sin i + \cos \delta \cos i \sin (\alpha - \text{N})] \\ &\qquad + \cos u \cos \delta \cos (\alpha - \text{N}). \end{aligned}$$

These equations are perfectly rigorous, and determine σ and p without ambiguity.

As the angle σ cannot be compared directly with observation, it will be more convenient to express σ in terms of s; thus from (6) we have

$$\tan s = \frac{r \sin \sigma}{r \cos \sigma + \Delta} \quad \ldots \quad \ldots \quad \ldots \quad (8)$$

and since s is always very small, we may substitute the arc for the tangent and develope the second member into a series. Hence we have

$$s = \frac{r}{\Delta} \sin \sigma - \tfrac{1}{2} \frac{r^2}{\Delta^2} \sin 2\sigma + \tfrac{1}{3} \frac{r^3}{\Delta^3} \sin 3\sigma - \quad \text{etc.} \quad \ldots \quad (9)$$

Now r is always very small compared with Δ, and therefore the terms containing the square and higher powers of $\frac{r}{\Delta}$ may be neglected without sensibly affecting the result. We shall then have

$$s'' = \frac{r}{\Delta \sin 1''} \sin \sigma \quad \ldots \quad \ldots \quad \ldots \quad (10)$$

and if a'' be the value of $\frac{r}{\sin 1''}$ at distance unit $\frac{a''}{\Delta}$ will be its value at distance Δ; therefore the last equation may be written

$$s'' = \frac{a''}{\Delta} \sin \sigma,$$

whence

$$\sin \sigma = \frac{\Delta}{a''} . s'' \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (11)$$

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which, substituted in the first two equations of (7), gives

$$s'' \sin p = \frac{a''}{\Delta}\left\{\sin u \cos i \cos(\alpha - N) - \cos u \sin(\alpha - N)\right\} \quad \ldots \quad \ldots \quad (12)$$

$$s'' \cos p = \frac{a''}{\Delta}\left\{\sin u\,[\cos\delta \sin i - \sin\delta \cos i \sin(\alpha - N)] - \cos u \sin\delta \cos(\alpha - N)\right\}$$

The error committed in s'' by omitting the second term of (9) is

$$\tfrac{1}{2}\frac{r^2}{\Delta^2}\,\frac{\sin 2\sigma}{\sin 1''}$$

which is a maximum when

$$\sigma = 45^\circ \text{ or } 135^\circ.$$

Put

$$\tfrac{1}{2}\frac{r^2}{\Delta^2}\,\frac{1}{\sin 1''} = \eta;$$

then

$$\frac{r}{\Delta} = \sqrt{\eta \sin 2''};$$

but $\frac{r}{\Delta}$ is the tangent of the angle of greatest elongation as seen from the earth; hence, putting

$$\frac{r}{\Delta} = \tan \epsilon,$$

we have

$$\eta = \frac{\tan^2 \epsilon}{\sin 2''},$$

when

$\epsilon = 0^\prime\ 50^{\prime\prime}$,	$\eta = 0''{\cdot}006$
$\epsilon = 1\ 50$,	$\eta = 0{\cdot}029$
$\epsilon = 3\ 35$,	$\eta = 0{\cdot}112$
$\epsilon = 5\ 46$,	$\eta = 0{\cdot}290$
$\epsilon = 9\ 45$,	$\eta = 0{\cdot}829$

Thus we see that for the satellites of *Mars*, *Uranus*, *Neptune*, the three inner satellites of *Jupiter*, and the five inner satellites of *Saturn* the error committed in s'' is less than $0''{\cdot}3$, and for *Callisto*, the fourth satellite of *Jupiter*, which has the largest angle of elongation, the error can never exceed $0''{\cdot}83$. In order to adopt (7) and (12) to logarithmic computation put

$$\begin{aligned} \sin f \cos F &= \cos i \cos(\alpha - N) \\ \sin f \sin F &= -\sin(\alpha - N) \quad \ldots \quad \ldots \quad \ldots \quad (13) \\ \cos f &= -\sin i \cos(\alpha - N) \end{aligned}$$

$$\begin{aligned} \sin g \cos G &= \cos\delta \sin i - \sin\delta \cos i \sin(\alpha - N) \\ \sin g \sin G &= -\sin\delta \cos(\alpha - N) \quad \ldots \quad \ldots \quad (14) \\ \cos g &= \cos\delta \cos i + \sin\delta \sin i \sin(\alpha - N) \end{aligned}$$

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$$\sin h \cos H = \sin \delta \sin i + \cos \delta \cos i \sin (\alpha - N)$$
$$\sin h \sin H = \cos \delta \cos (\alpha - N) \quad \ldots \quad \ldots \quad \ldots \quad (15)$$
$$\cos h = \sin \delta \cos i - \cos \delta \sin i \sin (\alpha - N).$$

Hence (7) and (12) become

$$\sin \sigma \sin p = \sin f \sin (F + u)$$
$$\sin \sigma \cos p = \sin g \sin (G + u) \quad \ldots \quad \ldots \quad \ldots \quad (16)$$
$$\cos \sigma = \sin h \sin (H + u)$$

and

$$s'' \sin p = \frac{a''}{\Delta} \sin f \sin (F + u)$$
$$s'' \cos p = \frac{a''}{\Delta} \sin g \sin (G + u) \quad \ldots \quad \ldots \quad (17)$$

The last group, which is known as Bessel's formulæ, gives s'' and p for any given date, and also enables us to compare the computed place with that obtained by observation. When s and p are to be computed for a series of dates it will be most convenient to compute the values of the auxiliary angles, f, F, g, and G, for the mean noon of several consecutive days, and then interpolate for any intermediate date. The auxiliaries h and H will not be required unless we also want the value of σ.

The following relations among the auxiliary angles may be here noticed, since they will be required presently in the reduction of the differential equations.

By combining the values of $\sin f \sin F$, $\sin g \cos G$, $\sin f \cos F$, and $\sin g \sin G$, we have

$$\sin f \sin g \sin F \cos G = \sin^2 (\alpha - N) \cos i \sin \delta - \sin (\alpha - N) \sin i \cos \delta$$
$$\sin f \sin g \cos F \sin G = -\cos^2 (\alpha - N) \cos i \sin \delta;$$

therefore by subtraction we have

$$\sin f \sin g \sin (F - G) = \cos i \sin \delta - \sin (\alpha - N) \sin i \cos \delta$$
$$= \cos h \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (18)$$

In a similar manner we find

$$\sin g \sin h \sin (G - H) = \cos f \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (19)$$
$$\sin f \sin h \sin (H - F) = \cos g \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (20)$$

Again, multiplying the first and second of (15) by $\cos u$ and $\sin u$ respectively, we have, after obvious reductions—

$$\sin h \cos (H + u)$$
$$= [\sin i \sin \delta + \cos i \cos \delta \sin (\alpha - N)] \cos u - \cos \delta \cos (\alpha - N) \sin u;$$

also, multiplying the second members of the first and second of (16) by the third of (14) and (13) respectively, we have, by the aid of (7)—

$$\cos f \sin g \sin (G + u) - \sin f \cos g \sin (F + u) = [\sin i \sin \delta$$
$$+ \cos i \cos \delta \sin (\alpha - N)] \cos u - \cos \delta \cos (\alpha - N) \sin u.$$

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Comparing this with the preceding equation, we have the following relation :

$$\sin h \cos (H+u) = \cos f \sin g \sin (G+u) - \sin f \cos g \sin (F+u) \quad (21)$$

To Correct the Elements.

When the computed places do not agree with those obtained by observation it will be necessary to correct the elements by forming equations of condition, which will be of the form

$$\Delta p = \frac{dp}{du}\Delta u + \frac{dp}{d\mathrm{N}}\Delta \mathrm{N} + \frac{dp}{di}\Delta i$$

$$\Delta s = \frac{ds}{du}\Delta u + \frac{ds}{d\mathrm{N}}\Delta \mathrm{N} + \frac{ds}{di}\Delta i + \frac{ds}{da}\Delta a,$$

where we consider for the present the elements of a circular orbit.

Differentiating the first of (7) we get (α and δ being constants)

$$\begin{aligned}\sin\sigma\cos p\,dp + \cos\sigma\sin p\,d\sigma = {} & [\sin(\alpha-\mathrm{N})\sin u + \cos(\alpha-\mathrm{N})\cos i\cos u]\,du \\ & + [\cos(\alpha-\mathrm{N})\cos u + \sin(\alpha-\mathrm{N})\cos i\sin u]\,d\mathrm{N} \\ & - \cos(\alpha-\mathrm{N})\sin i\sin u\,di \\ = {} & \sin f\cos(\mathrm{F}+u)\,du \\ & + [\cos(\alpha-\mathrm{N})\cos u \\ & \qquad + \sin(\alpha-\mathrm{N})\cos i\sin u]\,d\mathrm{N} \quad (a) \\ & + \cos f\sin u\,di\end{aligned}$$

Differentiating the second of (7) gives

$$\begin{aligned}-\sin\sigma\sin p\,dp + \cos\sigma\cos p\,d\sigma = {} & [\cos(\alpha-\mathrm{N})\sin\delta\sin u - \sin(\alpha-\mathrm{N}) \\ & \qquad \cos i\sin\delta\cos u + \sin i\cos\delta\cos u]\,du \\ & + [\cos(\alpha-\mathrm{N})\cos i\sin\delta\sin u \\ & \qquad - \sin(\alpha-\mathrm{N})\sin\delta\cos u]\,d\mathrm{N} \\ & + [\sin(\alpha-\mathrm{N})\sin i\sin\delta\sin u \\ & \qquad + \cos i\cos\delta\sin u]\,di \\ = {} & \sin g\cos(\mathrm{G}+u)\,du \\ & + \sin\delta\sin f\sin(\mathrm{F}+u)\,d\mathrm{N} \quad \ldots \quad (b) \\ & + \cos g\sin u\,di\end{aligned}$$

Multiplying (a) by the second of (16), and (b) by the first of (16), and subtracting the latter result from the former, we have, after reducing by the aid of the relations (18–21)—

$$\begin{aligned}\sin^2\sigma\,dp = {} & -\cos h\,du \\ & + [\sin i\sin h\sin(\mathrm{H}+u) - \sin\delta]\,d\mathrm{N} \quad \ldots \quad (22) \\ & + \sin h\cos(\mathrm{H}+u)\sin u\,di.\end{aligned}$$

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Differentiating the third of (7), we find after similar reductions

$$\begin{aligned}\sin\sigma d\sigma = &-\sin h\cos(\mathrm{H}+u)du\\ &+\cos\delta\sin f\sin(\mathrm{F}+u)d\mathrm{N}\\ &-\cos h\sin u di.\end{aligned} \quad \ldots \quad \ldots \quad (23)$$

From the plane triangle E P S (fig. 2) we have

$$\Delta = \Delta\cos s + r\cos(\sigma - s)$$

and

$$\sin s = \frac{r}{\Delta}\sin(\sigma - s).$$

Differentiating the latter and reducing by the former we have

$$\frac{\sin\sigma}{\sin s}ds = \cos(\sigma-s)d\sigma + \sin(\sigma-s)\frac{dr}{r} \quad \ldots \quad \ldots \quad (24)$$

whence

$$d\sigma = \frac{\sin\sigma}{\sin s\cos(\sigma-s)}ds - \tan(\sigma-s)\frac{da}{a},$$

where we write a for r in the circular orbit.

This being substituted in (23) gives, after putting for brevity

$$\sin s\cos(\sigma-s) = n,$$

$$\begin{aligned}\sin^2\sigma ds = &-n\sin h\cos(\mathrm{H}+u)du\\ &+n\cos\delta\sin f\sin(\mathrm{F}+u)d\mathrm{N}\\ &-n\cos h\sin u di\\ &+\frac{n}{a}\sin\sigma\tan(\sigma-s)da\end{aligned} \quad \ldots \quad \ldots \quad (25)$$

Therefore the partial differential coefficients of h and s with respect to the elements of the circular orbit are

$$\begin{aligned}\sin^2\sigma\frac{dp}{du} &= -\cos h\\ \sin^2\sigma\frac{dp}{d\mathrm{N}} &= \sin i\sin h\sin(\mathrm{H}+u)\sin u - \sin\delta\\ \sin^2\sigma\frac{dp}{di} &= \sin h\cos(\mathrm{H}+u)\sin u,\end{aligned} \quad \ldots \quad (26)$$

and

$$\begin{aligned}\sin^2\sigma\frac{ds}{du} &= -n\sin h\cos(\mathrm{H}+u)\\ \sin^2\sigma\frac{ds}{d\mathrm{N}} &= n\cos\delta\sin f\sin(\mathrm{F}+u)\\ \sin^2\sigma\frac{ds}{di} &= -n\cos h\sin u,\\ \sin^2\sigma\frac{ds}{da} &= \frac{n}{a}\sin\sigma\tan(\sigma-s),\end{aligned} \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (27)$$

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where s and a are to be expressed in seconds of arc at distance Δ.

If, however, we take the eccentricity of the orbit into consideration, we must express du and da or dr in terms of the remaining elements of the elliptic orbit. Let

m_o denote the mean anomaly at any given epoch T_o,

μ the mean motion in a solar day,

ϕ the angle of eccentricity such that $\sin\phi = e$,

E the eccentric anomaly,

v the true anomaly,

w the angular distance from the ascending node to the pericentre,

and

m the mean anomaly for any subsequent date t;

then by the theory of elliptic motion we have

$$\begin{aligned} m &= m_o + (t - T_o)\mu \\ &= E - \sin\phi \sin E \end{aligned}$$

$$E = m + (\sin\phi - \tfrac{1}{8}\sin^3\phi + \ldots)\sin m + \tfrac{1}{2}\sin^2\phi \sin 2m - \ldots\ldots$$

$$\cos E = -\tfrac{1}{2}\sin\phi + (1 - \tfrac{5}{8}\sin^2\phi + \ldots)\cos m + \tfrac{1}{2}\sin\phi\cos 2m - \ldots\ldots$$

$$\begin{aligned} r &= a(1 - \sin\phi\cos E) \\ &= a(1 - \sin\phi\cos m + \tfrac{1}{2}\sin^2\phi - \ldots) \quad \ldots \quad (28) \end{aligned}$$

$$v = m + 2\sin\phi\sin m + \tfrac{5}{4}\sin^2\phi\sin 2m + \ldots$$

and also

$$\begin{aligned} u &= w + v \\ &= w + m + 2\sin\phi\sin m + \ldots\ldots \quad \ldots \quad (29) \end{aligned}$$

Since the eccentricity of the orbits of all the satellites of the superior planets is not only small but also very uncertain, we may neglect the square and higher powers of $\sin\phi$ without sensibly affecting the accuracy of our results.

Differentiating (29) gives

$$du = dw + dm + 2\sin\phi\cos m dm + 2\sin m\cos\phi d\phi;$$

and expressing the radius vector of the satellite in terms of the semi-major axis (a) of its orbit by putting

$$\frac{r}{a} = \rho,$$

we shall have by differentiating this last equation

$$\frac{dr}{r} = \frac{da}{a} + \frac{d\rho}{\rho},$$

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where r and a are expressed in the same unit as Δ. Therefore from (28) we have

$$d\rho = d\left(\frac{r}{a}\right) = \sin\phi \sin m dm - \cos m \cos\phi d\phi;$$

hence

$$\frac{dr}{r} = \frac{da}{a} + \frac{\sin\phi \sin m}{\rho} dm - \frac{\cos m \cos\phi}{\rho} d\phi.$$

Therefore by substituting these values of du and $\frac{dr}{r}$ in (22), (24), and (25) we have for the partial differential coefficients of p and s with respect to the elements of the elliptic orbit the following results:—

$$\sin^2\sigma \frac{dp}{dw} = -\cos h$$

$$\sin^2\sigma \frac{dp}{dm} = -(1 + 2\sin\phi\cos m)\cos h$$

$$\sin^2\sigma \frac{dp}{d\phi} = -2\sin m\cos\phi\cos h \quad \ldots \quad \ldots \quad \ldots \quad (30)$$

$$\sin^2\sigma \frac{dp}{dN} = \sin i \sin h \sin(H+u)\sin u - \sin\delta$$

$$\sin^2\sigma \frac{dp}{di} = \sin h \cos(H+u)\sin u$$

and

$$\sin^2\sigma \frac{ds}{dw} = -n\sin h\cos(H+u)$$

$$\sin^2\sigma \frac{ds}{dm} = -(1+2\sin\varphi\cos m)n\sin h\cos(H+u) + \frac{n}{\rho}\sin\sigma\tan(\sigma-s)\sin\phi\sin m$$

$$\sin^2\sigma \frac{ds}{d\phi} = -[2n\sin m\sin h\cos(H+u) + \frac{n}{\rho}\sin\sigma\tan(\sigma-s)\cos m]\cos\phi$$

$$\sin^2\sigma \frac{ds}{dN} = n\cos\delta\sin f\sin(F+u) \quad \ldots \quad \ldots \quad \ldots \quad (31)$$

$$\sin^2\sigma \frac{ds}{di} = -n\cos h\sin u$$

$$\sin^2\sigma \frac{ds}{da} = \frac{n}{a}\sin\sigma\tan(\sigma-s),$$

in which a is to be expressed in seconds of arc at distance Δ.

To Compute the Times of greatest Eastern and Western Elongation.

Let us conceive three planes to be passed as follows:—

The first through the centre of the planet and the poles of the equator and the orbit of the satellite;

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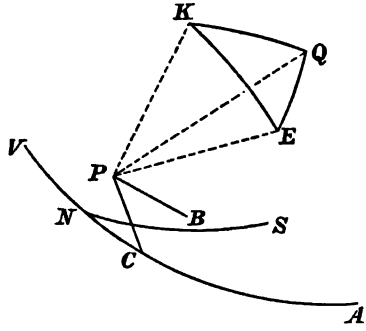
The second through the centre of the planet, the pole of the orbit of the satellite, and the centre of the Earth;

The third through the centre of the planet, the pole of the equator, and the centre of the Earth.

These three planes will determine on the celestial sphere a spherical triangle having its centre at the centre of the planet.



Let V A represent an arc of the equator, N S an arc of the orbit of the satellite, Q the pole of the equator, K the pole of the orbit of the satellite, N the position of the ascending node, P the planet, E the Earth.

Join P E, P K, P Q, and in the plane K P E draw P B perpendicular to K P, and P C perpendicular to P E; then P B will lie in the plane of the orbit, and P C in the plane of the apparent orbit, which is the projection of the real orbit on the plane passing through P, and perpendicular to the line P E joining the Earth and planet.

Put

θ = the angle EPB,

q = the angle of position of the minor axis of the apparent orbit,

$p_0 = q \pm 90°$, the angle of position of the major axis,

u_0 = the angular distance of the end of the major axis from the ascending node measured on the orbit of the satellite,

a and b = the semi major and minor axes respectively of the apparent orbit;

then in the spherical triangle E K Q we shall have

KQ = i, the inclination of the orbit of the satellite,

EQ = $90° + \delta$, the polar distance of the Earth as seen from the planet, and

EQK = the difference between the Right Ascension of the Earth and the Right Ascension of the pole of the orbit of the satellite

$= (\alpha - 180°) - (N - 90°)$

$= \alpha - N - 90°$;

hence the remaining three parts of the triangle can be found.

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Since the planes P K E, P K Q pass through the pole of the orbit, they are perpendicular to its plane, and the former intersects the orbit in those points which, as seen from the Earth, are nearest the planet, or, in other words, the intersection of the plane P K E with the apparent orbit, determining the minor axis, while the plane P K Q, being perpendicular to the planes of the equator and of the orbit of the satellite, intersects the latter at a point 90° distant from the node. The angle E K Q, being the inclination of these two planes, is measured by the arc of the orbit intercepted between these two points of intersection.

The side K Q will intersect the orbit in a point whose position is

$$90° + N;$$

and if we denote the position of the intersection of the side K E with the orbit by β we shall have

$$\text{angle EKQ} = 90° + N - \beta.$$

But since the plane P K E intersects the apparent orbit in its minor axis, the position of that point of the orbit at which the satellite, as seen from the Earth, will be at its greatest eastern elongation will be

$$\beta - 90°,$$

and therefore the distance of this point from the node will be

$$\beta - 90° - N.$$

Hence we have

$$u_0 = \beta - 90° - N.$$

But we have shown that

$$\text{angle EKQ} = -\beta + 90° + N;$$

therefore the angle E K Q$= -u_0$.

The angle K E Q is the angle of position of the minor axis of the apparent orbit; hence

$$\text{angle KEQ} = q = p_0 - 90°.$$

The side K E is the angular distance of the Earth from the pole of the orbit, as seen from the planet, and is equal to the angle B P C; therefore we have

$$KE = 90° - \theta,$$

where θ is always to be taken between $+90°$ and $-90°$. When θ is *positive* the Earth will be on the *north* side of the plane of the orbit and the position angles will *increase*, and when θ is *negative* the Earth will be on the *south* side and the position angles will *decrease*.

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The fundamental formulæ of spherical trigonometry being applied to the triangle E K Q, we have

$$\begin{aligned}
\cos EK &= \cos KQ \cos EQ + \sin KQ \sin EQ \cos KQE \\
\sin EK \cos EKQ &= \sin KQ \cos EQ - \cos KQ \sin EQ \cos KQE \\
\sin EK \sin EKQ &= \sin EQ \sin KQE \\
\sin EK \cos KEQ &= \sin EQ \cos KQ - \cos EQ \sin KQ \cos KQE \\
\sin EK \sin KEQ &= \sin KQ \sin KQE.
\end{aligned}$$

But we have shown that

$$\begin{aligned}
KQ &= i, \\
EQ &= 90^\circ + \delta, \\
KQE &= \alpha - N - 90^\circ, \text{ which are known;}
\end{aligned}$$

and

$$\begin{aligned}
EK &= 90^\circ - \theta, \\
EKQ &= -u_o, \\
KEQ &= p_o - 90^\circ, \text{ which are to be found.}
\end{aligned}$$

Substituting these values of the sides and angles in the preceding formulæ, we have

$$\begin{aligned}
\sin\theta &= -\cos i \sin\delta + \sin i \cos\delta \sin(\alpha - N) \\
\cos\theta \cos u_o &= -\sin i \sin\delta - \cos i \cos\delta \sin(\alpha - N) \\
\cos\theta \sin u_o &= \cos\delta \cos(\alpha - N) \quad \ldots \quad (32) \\
\cos\theta \sin p_o &= \cos i \cos\delta + \sin i \sin\delta \sin(\alpha - N) \\
\cos\theta \cos p_o &= \sin i \cos(\alpha - N)
\end{aligned}$$

for the determination of θ, u_o, and p_o. The agreement of the two values of $\cos\theta$ and the one value of $\sin\theta$ will serve to check the accuracy of the computation. It now remains to transform these equations for logarithmic calculation.

If we put

$$\begin{aligned}
c \sin C &= \cos i \\
c \cos C &= \sin i \sin(\alpha - N),
\end{aligned}$$

$$\begin{aligned}
c' \sin C' &= \sin i \\
c' \cos C' &= -\cos i \sin(\alpha - N),
\end{aligned}$$

and remembering that for the greatest western elongation $-u_o$ becomes $-u_o + 180^\circ$, and p_o becomes $p_o + 180^\circ$, we shall have

$$\begin{aligned}
\sin\theta &= c \cos(C \mp \delta) \\
\cos\theta \cos u_o &= \pm c' \cos(C' + \delta) \\
\cos\theta \sin u_o &= \pm \cos\delta \cos(\alpha - N) \quad \ldots \quad \ldots \quad (33) \\
\cos\theta \sin p_o &= \pm c \sin(C + \delta) \\
\cos\theta \cos p_o &= \pm \sin i \cos(\alpha - N),
\end{aligned}$$

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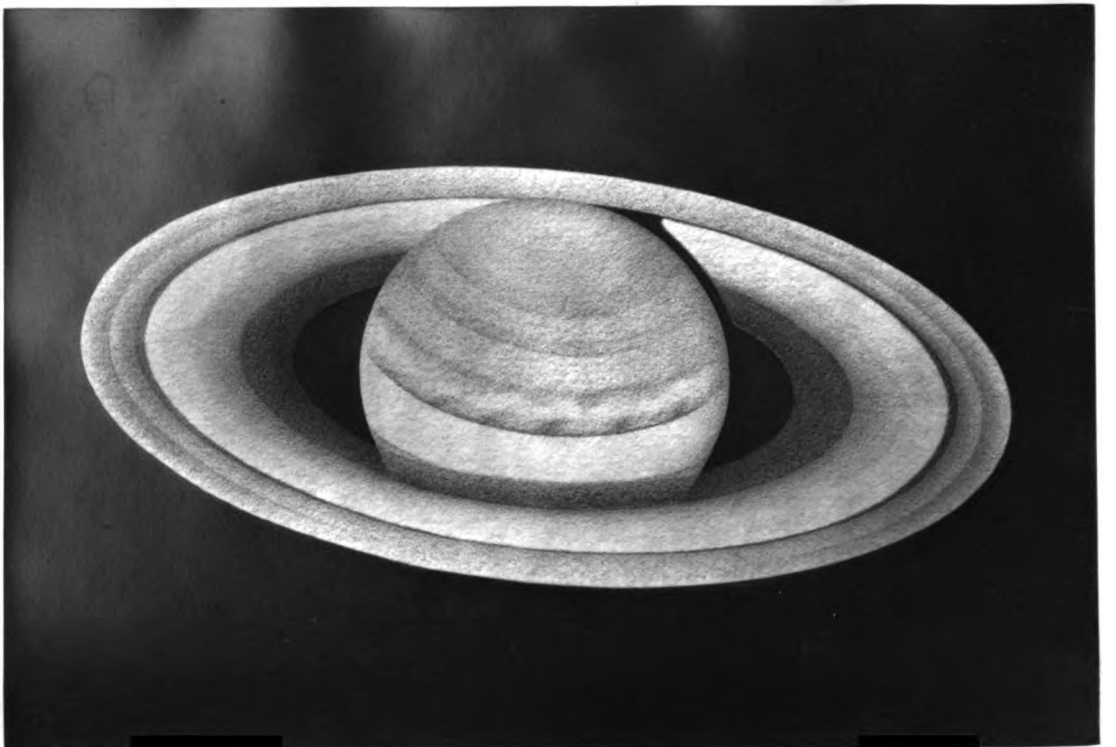

Henry Pratt, del. *Woodburytype.*

SATURN.

1884, *February* 11.

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in which the upper sign is to be used for the greatest eastern, and the lower sign for the greatest western elongation.

Now if u denote the distance of the satellite from the ascending node at the date T, and if t denote the time of greatest elongation, we shall have

$$(t-\mathrm{T})\,\mu = u_0 - u,$$

whence

$$t = \mathrm{T} + \frac{u_0 - u}{\mu} \quad \ldots \quad \ldots \quad \ldots \quad (34)$$

We also have

$$b = a \cos \mathrm{KE}$$
$$= a \sin \theta \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (35)$$

which determines the semi-minor axis of the apparent orbit.

The Physical Features of Saturn, 1884. By Henry Pratt.

The coincidence of a suitable position of *Saturn* with the occurrence of several nights when definition of a superlative character favoured me with opportunities for a minute examination of the planet, is my chief reason for offering to the Society the results of many observations. The occasions referred to were chiefly in February; some occurred in January and March; but the most notable occasion was Feb. 11, when I re-observed every detail I had previously seen.

The drawing which I then made to the scale of ·15 inch = 1″ was based upon micrometer measures, and a slightly reduced reproduction of it accompanies this paper.

The telescope used was my clock-driven Newtonian of 8·15 aperture. Its speculum, by Mr. With, gives remarkably small star disks, and in conjunction with that was used a reflecting prism of equally perfect form. The powers used have been 370, 450, and 500, chiefly the two last. All colour observations were made by Browning's achromatic power of 450.

Of the Globe.

The inclination of its axis towards the Earth on Feb. 11 was sufficient to render visible more than 20° of latitude beyond the south pole, bringing the parallel of 70° south latitude just within the southern limb.

The southern limb just covered Cassini's division.

The margin of the apparent disk of the globe generally appeared slightly deeper in tone than the more central portion, but the following limb was decidedly but delicately shaded, showing the slight approach of phase.

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Positions of the Dark Belts.

Changes doubtless occur in the form and arrangement of the belts on the globe, but from a constant and close examination of them through all the various aspects which the planet presents to us, I am forced to conclude that such real changes are by no means either frequent or rapid. For, whenever a slight alteration either in the form or position of the belts has been supposed to have occurred, I have afterwards found them resolved into their usual appearance by a more careful observation assisted by better definition.

Throughout the period during which the north pole was last turned towards the Earth, I observed the same regular arrangement of the belts on the northern hemisphere as was represented by the photograph of my drawing of 1874, Sept. 14, which accompanied my previous note "On the Belts of Saturn."

Throughout the period which has passed since the planet's southern pole last began to turn towards the Earth, I have constantly observed a similar arrangement of the belts on the southern hemisphere, but in reversed order to that presented by those on the northern one. And about the time when the ring was edgewise towards the Earth, the two symmetrical systems of belts were observed to occupy similar positions, so far as they could be traced north and south of the equator.

For the sake of future comparison I have designated the dark belts by the letters α, &c., commencing with the one nearest to the equator, and prefixing the minus sign to those in the southern hemisphere.

The equatorial zone has always appeared to me of a uniform tint without any markings whatever, and to extend for an equal distance on each side of the equator, its breadth being about 20° of Saturnian latitude. It is by far the most brilliant portion of the globe, and of a cream colour.

$-\alpha$. This belt, the first south of the equator, is the most interesting of all from its composite character. The position of its northern edge (as derived from micrometer measures) is 10° south latitude. Its general tint is reddish-brown, but for about one-fourth of its total breadth on its northern margin its tone is so intensified as to become a narrow dark line. The remainder of its breadth on the southern side is softened and modulated in tone, and often exhibits darker wispy knots. On Feb. 11 I counted six of such markings, three of them being quite easily seen.

$-\beta$, the next dark belt towards the south, is of the same tint as the former one, but much lighter in general tone, and is also mottled, but in a much more indistinct manner. This is especially the case on its northern margin, as to some extent it is a reversed counterpart of $-\alpha$, but, unlike the latter, it includes no darker markings.

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$-\gamma$ is a still fainter belt lying further south, its northern edge being usually the best defined.

$-\delta$, the fourth and last belt, is wider than $-\gamma$, and more difficult to see, and its margins are less defined. Both the last-mentioned belts are so faint as to be always invisible unless our atmosphere is in a very transparent condition. On favourable occasions I always see them, and, as far as it is possible to determine, their general colour is the same as that of the other belts.

Between each of the four belts above mentioned, as well as south of $-\delta$, are *zones* of lighter tone, and of a pale brownish-yellow colour, each more or less softly blended with the belts, and of a darker tone than the brilliant equatorial zone.

The polar region, lying entirely within view, is without any defined boundary, being blended very softly with the zone on its north, with which it is contrasted by its darker tone; while on its farther margin it is oppositely contrasted with the still darker southern limb. Its colour appears bluish-gray.

$+a$. On each side of the globe, the ends of this belt are just visible above the margin of the ring C, with which it is easily confused unless definition is of a fine order. It thus shows that the extent of the equatorial zone at the present time is about the same on the northern as it is on the southern side of the planet's equator.

Of the Rings.

A. The marking upon this ring, which is usually termed Encke's division, was very easily seen during February and March, sometimes being very unusually distinct, as it also appeared on some occasions in 1883. But it has never to my vision seemed like a simple sharply-defined line on an even flat tint, as sometimes it has been represented. It has always appeared to me very narrow and gray, with the character of a delicately softened linear shading, varying in depth of tone from time to time. I consider it to be not a complete division, and that it does not always occupy the same position on the ring. The brightness of the outer zone of A begins to diminish about its middle, and the depth of its tone increases until it meets Encke's division. And it is remarkable that on the inner zone is a repetition of the same phenomenon until it meets Cassini's division. These shadings, or whatever else they may be termed, are not unworthy of note. On 1881, Nov. 22, Encke's division was seen just within the middle of A, but the striated appearance of the shading on its outside during moments of best definition was such as to indicate the high probability of the existence of more than one marking of the nature of Encke's division; and on some nights in last January it appeared just outside of the middle of A, while on others I concluded (after much careful attention) that it was just outside of the middle on one ansa and just

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within it on the other. The general colour of A is usually pale rose-madder.

Cassini's division always appears very dark indeed, but under the most favourable conditions it is never a perfect black comparable with the shadow of the globe on the ring.

B. From the outer zone of this ring, which is always very brilliant, its tone is usually softly and gently graduated to its very dark inner margin, without the slightest indication of marked variations. But on some occasions I have seen very slight deviations from this regular progression of tone. On Feb. 11, immediately within the brilliant outer zone was one slightly darker. But within that, the brightness very slightly increased again for a short space. And within that, the intensity of tone gradually increased to the inner margin. Under a high power the inner margin appears so very ill defined and blended with C, that it is not easy to determine the boundary between the two. The closest scrutiny has never enabled me to detect a division between or upon either. The general tint of this ring appears bluish-gray.

C. The general colour of this ring appears to me slaty-gray, but tinted with purple on the ansæ. Its fairly definite interior edge is probably rendered the more perceptible, by the sudden contrast of the black sky within the ansæ. Its general tone becomes gradually but very decidedly darker from its exterior to its interior margin, except where it crosses the globe. There the reverse is the case, for it becomes apparently lighter towards its interior margin, where its outline is very soft and diffused, as if semi-transparent. But its whole breadth is perceptible on the globe, and the globe is seen through the whole breadth of the ring, with the slight exception of a very narrow zone including its outer margin. It is remarkable that on Feb. 11, and other occasions, it appeared brighter as well as slightly narrower on the preceding ansa than on the following one, as I have represented in the drawing. When definition is very fine, a sort of grain is decidedly visible over its whole extent, as if its ultimate particles were aggregated in groups just large enough to be seen by momentary glimpses. But although this effect cannot be seen persistently, it certainly cannot be represented by a uniformly flat wash of colour.

Eccentricity of Rings in reference to Globe.

When the differing breadths of the two ends of C were observed on Feb. 11, the inner margin of the preceding ansa was seen to be slightly nearer to the globe than was that of the following one. And on Feb. 29, at 9^h, this eccentricity appeared on the same side, and to a much greater extent than I had ever observed. Again, on March 2, at 9^h, the eccentricity appeared about midway between its quantities on the two former dates.

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Shadow of the Globe on the Rings.

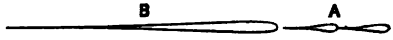
Several times during February I examined this carefully on account of its peculiar form. It appeared either to just meet or to cover Cassini's division, and followed it round towards the planet's polar limb without quite reaching it. The eastern margin of the shadow almost reproduced the outline of the globe, until near Cassini's division, when it curved sharply round towards the east and joined the division. No trace of the shadow was to be detected on the ring A.

On the probable Section of the Rings.

The extreme thinness of the rings, and the deviation of their surfaces from perfect planes, are matters of general observation.

The general form of those surfaces, as indicated by my observations of the shadow of the globe falling upon them in all positions of the ring, I have endeavoured to approximately determine, by critically examining in sunlight various models of the globe and rings which I had made to scale; the result being that *only one* form would yield (under the necessary conditions) artificial shadows in accord with all the real ones I have observed.

The section so obtained is here given.



On the Constitution of the Rings.

Observation and mathematical investigation alike indicate that the matter of which the rings are composed is in the condition of circulating streams of solid particles.

It has been matter of observation from the days of Short and Kater to the present time, that the outer ring A exhibits a more marked tendency to *striation* than the others. This I found very unusually visible on Nov. 22, 1881, and on different occasions since then. Similarly (only in recent times) the inner ring C has been found to exhibit the greatest tendency to *granulation*.

Observation has also shown (by comparison of the shadows of the globe) that the greatest thickness of each ring lies towards its outer margin.

To these results of observation may be added the consideration that the outer portion of the ring is doubtless occupied by its larger and heavier particles, and conversely, that its smaller and lighter ones compose its inner portion.

It then becomes interesting to note the following facts of observation:—

The maximum brightness coincides with the maximum thickness near the outer margin of B.

The maximum darkness of tone with the thinnest portion, which is the inner margin of C.

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The greatest tendency to striation, with the place of the heaviest particles, as in A.

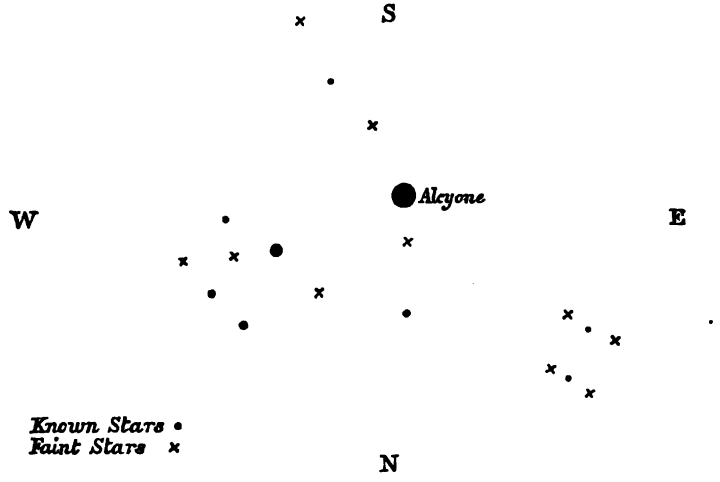
The greatest tendency to granulation, with the position probably occupied by the lightest particles, the inner ring C.

From this we may infer that the matter of which the ring B is composed, is less likely than that of the other rings, to develop distinctively either the character of granulation or of striation; and conclude that the gravitational influences which affect the ring system, produce slowly and continually all those variations of tone, tint, and structure which we are able to observe.

18 *Preston Street, Brighton.*

Faint Stars near Alcyone and near β^1 *and* β^2 *Capricorni.* By A. A. Common.

On the night of November 19, 1881, I examined the immediate neighbourhood of *Alcyone* with the three-foot Reflector and noted some faint stars. As they are in the centre of such an interesting group I have given a sketch showing their approximately relative positions to the well-known stars which are represented on the sketch by round dots, the position of the fainter stars being marked by crosses. The faintest of these is that nearly midway between two of the well-known three stars near *Alcyone*.



In August 1879, one of the first objects examined with this telescope, then just mounted, was the faint pair between β^1 and β^2 *Capricorni*, mentioned by Smyth in the *Bedford Catalogue*, p. 475, as having been discovered by Sir John Herschel, who considered it a delicate object, and remarked that no telescope

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that was not competent to divide this pair would have the remotest chance with the satellite of *Uranus*.

While examining this pair I found another of a similar character, but very much fainter, within a radius of, say, 2′ of arc of the known pair, the two pairs forming a very delicate test for a large aperture. All these faint stars are easily seen—that is to say, they are not mere glimpse objects in this telescope—and it would therefore be interesting to know if some of the larger Refractors show them with the same ease. I have not given the exact place of the pair, so as not to mislead an observer, but I shall be glad to compare places.

1884 *June* 10.

On a New Variable Star in Monoceros. By J. E. Gore, M.R.I.A.

On the evening of February 17, 1884, while estimating the magnitude of 19 *Monocerotis*, suspected of variation by Dr. Gould (*Uranometria Argentina*, p. 332), I was surprised to find that one of the comparison stars selected—Lalande 13627—estimated $5{\cdot}6^m$ at Cordoba, was only 6^m, and more than half a magnitude fainter than 20 *Monocerotis*, rated 5·4 in the *Uranometria Argentina*. As Dr. Gould's magnitudes are so very accurate I immediately suspected variation, but, owing to cloudy weather, I could not confirm my observation until February 21, when I estimated the star in question only 2 steps brighter than Lalande 13706 (6·2 Gould) *nf*.

I then determined to watch the star for variation, and I give the results of my observations on every clear night up to April 20, 1884, which was the last occasion on which I could observe the star. From these observations it will be seen that the star increased in brightness up to $5{\cdot}5^m$ on March 4. It then slowly diminished in light, and was again only 6^m on March 21. After that date it again increased, and was about $5{\cdot}6^m$ on April 8 and 9, since which date it again diminished, and was about $5{\cdot}9^m$ when last observed on April 20. A small variation would therefore seem to be certain, with a period of about ±35 days, but a more extended series of observations will of course be necessary to determine the exact period with accuracy.

The observed fluctuations of light would at first sight appear to be trifling, but we have a similar case in the well-known variable R (13) *Lyræ*, which only varies from 4·3 to 4·6, with a period of forty-six days, and in R *Eridani*, which varies from 5·4 to 6·0, according to the independent observations of three observers at Cordoba. Other cases of small variation are T *Carinæ* (6·2 to 6·9) and R *Muscæ* (6·6 to 7·4), discovered at Cordoba.

The star is reddish, and some of the observations may possibly have been slightly influenced by the presence of moonlight, although I do not find such to be the case in my observations of

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the well-known variable μ *Cephei*, which is a remarkably red star.

The comparison stars are—

		Mag. Gould.
(A) = 20 Monocerotis	... ...	5·4
(B) = Lalande 13627	... ...	5·6 supposed variable.
(C) = ,, 13339	... ...	5·9
(F) = ,, 13143	... ...	6·1
(D) = ,, 13706	... ...	6·2
(E) = ,, 13375	... ...	6·4
(G) = ,, 13104	... ...	6·2

19 *Monocerotis* I have variously estimated from 5·3 to 5·6 during the course of the following observations. I also suspect the star C of a very small fluctuation of light. The other comparison stars seem to be constant.

Closely *f* 19 *Monocerotis* is a small star, Lalande 13678 ($8\frac{1}{2}^m$), which was suspected of variation by Taylor. It was estimated 8^m at Cordoba, and Dr. Gould says, 'The supposition of variability may have arisen from mistaking it for F 19' (*Uranometria Argentina*, p. 333).

The place of Lalande 13627 is for 1884·0

	h m s
R.A.	6 56 15
Dec. S.	° ′ ″ 5 33 23

The observations were made by eye estimations with a binocular field-glass having object-glasses of 2 inches aperture, and a magnifying power of about 6 diameters.

Observations of Lalande 13627.

Date of Obs.	d	h	Est. Mag.	Remarks.
1884, Feb.	17		6·0	Very little brighter than D; more than $\frac{1}{2}$ mag. less than 20.
	21		6·0	2 steps brighter than D, but 6 steps less than 20 Monocerotis.
Mar.	3		5·8	Moonlight; midway in light between 20 and D.
	4	10	5·5	Only 1 step less than 20 Monocerotis.
	7	8	5·6	Moonlight.
	9	$9\frac{1}{2}$	5·7	,,
	10	$9\frac{1}{2}$	5·7	Strong moonlight; observation difficult.
	11	8	5·75	Strong moonlight and hazy sky.
	13	$10\frac{1}{2}$	5·8	,, ,,

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Date of Obs.	d	h	Est. Mag.	Remarks.
1884, Mar.	14	8	5·85	No moon.
	15	8½	5·9	„
	17	8¾	5·9	Fine night; no moon.
	18		5·8	Hazy sky.
	20	10¾	5·9	No moon.
	21		6·0	„
	23	8¼	5·9	„
	24	9½	5·8	Hazy sky.
	25			On this date the star was estimated 5·8 by Franks at Leicester with 11½-in. reflector.
	31	8¼	5·7	Hazy sky and moonlight.
Apr.	1		5·7	
	2		5·7	Strong and near moonlight.
	6	10½	5·7	Strong moonlight and star low.
	7		5·65	„ „
	8	8½	5·6	Strong moonlight.
	9	8¼	5·6	„
	10	9	5·7	„
	11	8¾	5·7	Hazy sky.
	12	9	5·7	Clear sky; no moon.
	17	9	5·8	No moon.
	20	9½	5·9	No moon; star near W. horizon.

Suggestions for the Improvement of the Transit Circle.

By Ralph Copeland, Ph.D.

In a recent paper containing suggestions for the construction of a large Transit Circle Mr. Common has mainly dealt with the stiffness and intimate connection of an axis and telescope of steel, contenting himself as regards the circle either with making it out of a single steel disk separately attached to the axis or forming part of the frame or girder by which it is proposed to replace the tube. As a whole Mr. Common's scheme may therefore be described as embracing a telescope and axis of enormous strength, obtained by the use of considerable quantities of steel, while all desirable mobility and relief from strain, in the axis at least, are to be secured by the use of mercurial counterpoises. Mr. Common's plans do not profess to go into detail, and thus leave in a great measure untouched several points in the construction of our larger Transit Circles that are perhaps quite as great sources of error as any want of

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stiffness in the axis. I think few people can contemplate a large Transit Circle with its objective cell held by sets of opposing centring screws, its eye end with movable Right Ascension wire plate crossed by a declination micrometer, and these in their turn, followed by "slipping" slides carrying the eyepiece across the field, perhaps in Declination as well as Right Ascension —few people, I think, can look at this accumulation of mechanism without asking if it really permits the wires to maintain their zero points with regard to the optical axis unaltered in all positions of the instrument. In the case of parallax or other delicate observations with the equatorial this would not be assumed for one moment; all the changes would be rung of telescope preceding, telescope following, micrometer screw north, and micrometer screw south. These are the precautions that would be taken in merely differential operations. They should hardly be disregarded in fundamental observations. Many of the risks of instrumental derangement alluded to can be avoided in a great measure by letting the object-glass directly into one end of the tube and the wire plate into the other. By continually testing the centring and focus during the progress of the work this need not be a difficult operation.* Something would doubtless also be gained by selecting a material for the tube such that its coefficient of expansion would be the same as that of the *focal length* of the object glass, a precaution taken many years ago by the late Prof. Bruhns in the construction of the Leipzig Refractor. Fraunhofer's well-known plan of securing the objective as described in *Ast. Nach.*, No. 59, is still in general use; it would be rendered more certain in its action by cementing the lenses together. The use of a perfectly plain, unbroken and unpierced cylindrical tube passing through and *soldered into* an axis sufficiently enlarged in the middle of its length was suggested to me by my friend Dr. David Gill in conversation some years ago. This promising device has the great advantage of extreme simplicity. The telescope thus reduced to its simplest elements of an object-glass, the fiducial wires (no matter whether these are spiders' webs or lines ruled on glass), and a plain supporting tube ought to be considered as something not further to be loaded or otherwise molested. I therefore propose to surround the same by an outer separate tube, deriving its support from the neighbourhood of the *point d'appui* of the counterpoises, which should carry all the illuminating apparatus, the eyepiece and its slides, as well as any micrometer that might be found desirable. Advantage might also be taken of such an arrangement to protect the tube from all sudden changes of temperature caused by currents of air or by radiation from the observer's body, particularly when making reflex and nadir observations. The oft-repeated proposal of using a glass disk in place of a

* Mr. Common's proposal to use a double telescope "where the O.G. of one is in front of the eyepiece of the other" would here be of the greatest practical value. See p. 293.

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graduated metal wheel offers several advantages beyond those usually claimed, chief among which may be reckoned the facility of illumination *through* the circle and the facility of adjusting the tubes of the microscopes perpendicular to its plane by means of reflections from its surface. The divisions could doubtless also be made much finer than on silver, which, for instance, in the Dunecht instrument are about 7″ in width.

Another point connected with the divided circles does not seem to have received all the attention it deserves. It is well known that many of the finer circles in use are so accurate that their division errors do not exceed the deformations to which they may be readily exposed by inequality of temperature in their different parts; for in a brass circle an arc of 60° is expanded about 1″ for 1° Fahr. Hence a difference of temperature in the upper and lower parts of a perfect circle to this extent would produce apparent division errors of very appreciable magnitude. It is to be feared that in many observatories much larger differences of temperature may be found in the neighbourhood of the Transit Circle at points not more distant from each other than the circle's diameter. In vol. i. of the *Annalen der Sternwarte in Leiden*, p. ciii., may be found an interesting account of the precautions observed by Pistor and Martins when engaged in graduating a circle, from which we learn that when a circle is mounted on the dividing engine the surrounding air is kept in motion by automatic fans for several hours before a single division is cut, and the circulation of the air is maintained as long as the work is in progress. If this is found necessary in the construction of a circle it is no less necessary in its most refined use. Perhaps by enclosing the circle in a box in which the air is kept in motion by a light fan driven by clockwork the chief requirements of the case would be met. Perhaps it might be found advantageous to surround the box by some bad conductor of heat. It is at least certain that no experimentalist would compare two thermometers immersed in a fluid without carefully stirring the same. In examining the division errors of one of the circles of the Dunecht meridian instrument it was found that the interposition of a milled board between the circle and the massive cast-iron pier head greatly improved the accordance of the observations, probably by cutting off the radiations from the pier. In the case, too, of reflex observations it has long been known that imperfect ventilation of the neighbourhood of the quicksilver trough seriously affected the value of the results. Here again some form of mechanical aid to ventilation seems to be called for. It is very desirable also that all the illuminating lamps should be banished once for all from the meridian room and its more immediate vicinity. By passing the needful beams of light through water they could be deprived of nearly all their heating effect.

Dunecht Observatory:
1884 *June* 10.

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Note on the Causes of the Blurred Patches in Instantaneous Photographs of the Sun. By A. C. Ranyard.

The beautiful photographs of the sun taken at Meudon by M. Janssen exhibit curious patches of a lighter shade within which the definition of the rice grain structure of the photosphere is interfered with. M. Janssen is inclined to attribute these patches to solar clouds or gaseous masses above the photosphere, which he supposes change their shape very rapidly. I have recently been making some experiments which show that similar indistinct lighter patches may be caused by currents of heated air in the tube of the telescope. The experiments have been made with a reflecting telescope with a heavy iron tube. If the image of a bright star is observed out of focus on an ordinary night bright ripples may be seen passing across the disk of light, which is really an image of the speculum with the flat projected on its centre. That these ripples are due to the unequal refraction of heated air currents may be proved by placing a heated body in the tube of the telescope. I made use of a heated iron chisel, which was placed in different parts of the mouth of the telescope, and was let down the tube by a string. This greatly increased the distinctness of the ripples, as well as the velocity with which they passed across the image of the speculum; the dark lines corresponding to the supports of the flat enabled one to localize the part of the tube from which the heated air was rising, and showed that it corresponded with the place where the hot piece of iron was held. Though these ripples are only seen when the star is out of focus, the differences of refraction caused by heated air currents must affect the sharpness of the image in focus, and give rise to scattered light round the image of a star. In the case of a uniformly bright disk heated air currents would evidently give rise to areas of greater and less brightness. In order to test this I took some instantaneous photographs of the sun with a piece of heated iron in the mouth of the telescope, and also when the sun was seen just above a heated roof. In both cases the limb of the sun as shown in the photographs is much distorted, and brighter areas are seen extending over the sun's disk. In the case of the photographs taken with the hot iron in the mouth of the telescope the brighter areas have a tree-like form.

The photographs were taken in a camera 7 ft. 6 in. long, attached to the side of an 18-in. telescope of 9 ft. focal length. The light is reflected from the flat of the telescope as in the ordinary Newtonian form, and again reflected by a second flat into the camera, which is provided with a photographic lens giving an image of the sun nearly thirty inches in diameter.

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Observations of Occultations of Stars by the Moon, 1883–4, and of Phenomena of Jupiter's Satellites, 1884, made at the Radcliffe Observatory, Oxford.

(*Communicated by E. J. Stone, M.A., F.R.S., Radcliffe Observer.*)

Occultations of Stars by the Moon.

Ref.	Day of Observation.	Phenomenon.	Moon's Limb.	Instrument.	Power.	G.M.T. of Obs. h m s	Observer.
(*a*)	1883, July 17	Disapp. B.A.C. 6081	Dark	Heliometer	200	11 28 52·07	W.
(*b*)	Aug. 24	Reapp. B.A.C. 1206	,,	42-inch Telescope	80	11 21 59·97	R.
(*c*)	Nov. 19	,, A^2 Cancri	,,	,,	80	12 18 38·44	R.
(*d*)	1884, Mar. 6	Disapp. λ Geminorum	,,	10-foot Equatorial	160	10 7 35·24	R.
(*e*)	6	Reapp. λ Geminorum	Bright	,,	160	11 14 6·97	W.

Remarks.

(*a*) Sky hazy and star very faint.
(*b*) and (*c*) Observations good.
(*d*) Instantaneous; observation good.
(*e*) Appeared gradually, taking about one second to acquire full brightness.

Phenomena of Jupiter's Satellites.

Ref.	Day of Observation.	Satellite.	Phenomenon.	Instrument.	Power.	G.M.T. of Obs. h m s	G.M.T. of N.A. h m s	Observer.
	1884, Feb. 13	III.	Tr. E. First contact	10-foot Equatorial	160	7 28 34	7 35 0	R.
			Bisection	,,	,,	7 32 59		
			Last contact	,,	,,	7 37 48		
(*a*)	15	I.	Ecl. R. First seen	,,	,,	12 5 20	12 5 28	,,
(*b*)	21	II.	Ecl. R. First seen	,,	,,	8 16 45	8 16 9	F.B.

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Ref.	Day of Observation.	Satellite.	Phenomenon.	Instrument.	Power.	G.M.T. of Obs. h m s	G.M.T. of N.A. h m s	Observer.
	1884, Mar. 17	I.	Tr. I. Ext. contact	10-foot Equatorial	160	8 5 27		
			Bisection	,,	,,	8 9 26	8 8 0	R.
			Last contact	,,	,,	8 13 56		
(*b'*)	18	I.	Ecl. R. First seen	,,	,,	8 43 20	8 43 27	F.B.
	21	IV.	Ecl. R. Suspected	,,	125	8 17 32		
			Decided	,,	,,	8 17 47	8 22 19	R.
			Full brightness	,,	,,	8 33 44		,,
(*c*)	21	IV.	Ecl. R. First trace	Heliometer	160	8 17 36		
			Fully established	,,	,,	8 17 44	8 22 19	F.B.
			Full brightness	,,	,,	8 35 9		,,
	24	II.	Ecl. R. First seen	10-foot Equatorial	125	7 56 3	7 56 6	R.
(*d*)			As bright as J.I.	,,	,,	7 59 38		,,
			Full brightness	,,	,,	8 1 38		,,
	24	II.	Ecl. R. As bright as J.I.	Heliometer	160	7 59 51		F.B.
(*d'*)	24	I.	Tr. I. Ext. contact	10-foot Equatorial	125	9 57 19		
			Bisection	,,	,,	10 1 3	9 59 0	R.
			Int. contact	,,	,,	10 5 18		
(*e*)	Apr. 10	I.	Ecl. R. First seen	Heliometer	160	8 58 52	8 58 36	W.
(*f*)	10	I.	Ecl. R. First seen	10-foot Equatorial	125	8 59 20	8 58 36	F.B.
			Full brightness	,,	,,	9 0 50±		,,
(*g*)	10	III.	Tr. I. First contact	Heliometer	160	9 6 41		
			Bisection	,,	,,	9 13 50	9 12 0	W.
			Last contact	,,	,,	9 21 39		

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Ref.	Day of Observation.	Satellite.	Phenomenon.	Instrument.	Power.	G.M.T. of Obs. h m s	G.M.T. of N.A. h m s	Observer.
	1884, Apr. 10	III.	Tr. I. First contact	10-foot Equatorial	125	9 8 16		
			Bisection	,,	,,	9 13 25	9 12 0	F.B.
			Last contact	,,	,,	9 19 54		
(*h*)	21	III.	Ecl. D. Last seen	Heliometer	160	8 25 13	8 23 10	,,
(*i*)	21	III.	Ecl. R. First seen	10-foot Equatorial	125	11 54 37	11 53 55	R.
			Half as bright as J.I.	,,	,,	11 57 32		,,
			As bright as J.I.	,,	,,	11 59 32		,,
			Full brightness	,,	,,	12 0 18		,,
(*k*)	May 5	III.	Occ. D. First contact	,,	125	11 13 49		
			Bisection	,,	,,	11 18 48	11 23 0	,,
			Last contact	,,	,,	11 23 17		
(*l*)	10	I.	Ecl. R. First seen	,,	,,	11 9 21	11 9 21	F.B.

Remarks.

(*a*), (*b*), (*c*), (*d'*), (*f*), and (*l*). Images good. (*b'*) The time noted has been increased by 5m.
(*d*) At this time the two satellites (I. and II.) appeared like a very close double star of equal magnitudes.
(*e*) May be a second or two late. (*g*) Image very good at times. (*h*) Good; disappeared quicker than usual.
(*i*) Time of first seen very probably late. Some light lost; *Jupiter* and his satellites observed through trees. Not satisfactory.
(*k*) *Jupiter's* limb very ill-defined. Satellite rather faint.

In converting Oxford Mean Solar Times into Greenwich Mean Solar Times the following Longitude has been used: 5m 2s·6 W.

Observers: W. = Mr. Wickham. R. = Mr. Robinson. F.B. = Mr. Bellamy.

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Ephemerides of the Satellites of Saturn, 1884–85.

By A. Marth.

The following ephemerides of the five inner satellites are founded upon the elements which have served in the preparation of the ephemerides for the last apparitions of *Saturn*, as the results of some observed conjunctions of the satellites with the centre of the planet indicate that the adopted elements are sufficiently near the truth for the present purpose. The five satellites deviate so little from the plane of the ring that it will be most suitable to treat their deviations as latitudes above this plane, the ascending node N and inclination I of which in reference to the plane of the Earth's equator are here assumed to be for 1885·0

$$N = 126^\circ.5128 \qquad I = 7^\circ.0059.$$

The assumed longitudes of the satellites in their orbits (*i.e.* their longitudes from the ascending node added to the Right Ascension N of the ascending node reckoned from the true point of the equinox) referred to the time when the light arrives at the distance [0·950], or at the distance the logarithm of which is 0·950, are the following:

0h Greenwich.	Mimas.	Enceladus.	Tethys.	Dione.	Rhea.
1884, Aug. 21	126°.501	89°.357	93°.634	275°.252	157°.529
Sept. 20	66·244	51·314	54·578	261·304	28·234
Oct. 20	5·989	13·270	15·522	247·355	258·938
Nov. 19	305·735	335·227	336·467	233·407	119·643
Dec. 19	245·482	297·183	297·411	219·459	0·347
1885, Jan. 18	185·231	259·140	258·356	205·511	231·052
Feb. 17	124·980	221·097	219·300	191·562	101·756
Mar. 19	64·700	183·053	180·245	177·614	332·461
Apr. 18	4·481	145·010	141·189	163·665	203·165

The adopted daily sidereal motions of the five satellites and the corresponding times of their sidereal revolutions are:

		d h m s
Mimas	381°.99146	0 22 37 5·91
Enceladus	262·73186	1 8 53 6·86
Tethys	190·69812	1 21 18 25·96
Dione	131·53503	2 17 41 9·33
Rhea	79·69012	4 12 25 11·87

The values for *Mimas* belong to Sept. 20, 1884, and depend on the hypothesis of accelerated motion introduced in the Ephemeris for 1880.

In the following tables P denotes the position-angle of the

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minor axis of the ring; L+180° the planetocentric longitude of the Earth referred to the plane of the ring; Λ+180° that of the Sun, or Λ−L the difference between the two. The apparent equatorial diameter of the ball and the diameter of the outer rim of the ring depend still on Bessel's determinations. The assumed proportion of the polar axis of the ball to the equatorial diameter is=0·900.

In the tables for the satellites a and b are the semi-axes of the apparent orbits, $l-L$ the longitudes of the satellites in their orbits reckoned from the points which are in superior conjunction with the planet's centre or in opposition to the Earth in longitude. By adding to $l-L$ the value of L from the first table the longitudes l are found.

The values of P, a, b and $l-L$ are to be interpolated for the times for which the apparent positions of the satellites are required, and the rectangular coordinates x and y, reckoned parallel to the axes of the ring and expressed in seconds of arc, or, if polar coordinates are wanted, the position-angles p and distances s of the satellites in reference to the centre of the planet are then found by means of the equations

$$s \sin (p-P) = x = a \sin (l-L)$$
$$s \cos (p-P) = y = b \cos (l-L).$$

[b]Gr.	P.	L.	Latitude of Earth above plane of ring.	Latitude of Sun above plane of ring.	Λ−L.
1884.	°	°	°	°	°
Aug. 31	354·361	84·892	−26·615	−26·697	−6·933
Sept. 5	·332	85·215	·595	·705	7·048
10	·308	85·492	·576	·713	7·117
15	·288	85·723	·559	·721	7·139
20	·273	85·906	·543	·728	7·113
25	·262	86·039	·530	·735	7·038
30	·254	86·122	·519	·742	6·912
Oct. 5	354·252	86·153	−26·511	−26·749	−6·734
10	·254	86·133	·506	·755	6·505
15	·261	86·061	·504	·761	6·225
20	·272	85·939	·505	·767	5·894
25	·287	85·766	·509	·772	5·513
30	·307	85·546	·516	·777	5·084
Nov. 4	354·330	85·282	−26·525	−26·782	−4·611
9	·357	84·976	·536	·786	4·096
14	·388	84·631	·550	·790	3·542
19	·423	84·253	·565	·794	2·955
24	·460	83·847	·582	·798	2·339
29	·499	83·418	·599	·801	1·701

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0^h Gr.		P.	L.	Latitude of Earth above plane of ring.	Latitude of Sun above plane of ring.	A−L.
1884.		°	°	°	°	°
Dec.	4	354·540	82·971	−26·617	−26·804	−1·046
	9	·583	82·515	·635	·806	−0·380
	14	·626	82·054	·653	·808	+0·290
	19	·670	81·596	·671	·809	0·957
	24	·712	81·147	·689	·810	1·615
	29	·754	80·714	·707	·811	2·257
1885.						
Jan.	3	354·794	80·305	−26·724	−26·812	+2·876
	8	·831	79·923	·741	·813	3·467
	13	·865	79·574	·758	·813	4·025
	18	·895	79·262	·775	·814	4·546
	23	·921	78·994	·792	·813	5·024
	28	·943	78·771	·809	·812	5·456
Feb.	2	354·960	78·596	−26·827	−26·811	+5·840
	7	·972	78·471	·845	·810	6·174
	12	·979	78·398	·863	·809	6·456
	17	·980	78·378	·882	·808	6·685
	22	·976	78·412	·901	·806	6·861
	27	·967	78·498	·920	·804	6·984
Mar.	4	354·953	78·636	−26·939	−26·801	+7·055
	9	·933	78·825	·958	·798	7·075
	14	·908	79·063	·976	·795	7·047
	19	·879	79·348	26·994	·791	6·971
	24	·846	79·680	27·011	·787	6·847
	29	·808	80·058	27·026	·783	6·679
Apr.	3	354·767	80·477	−27·039	−26·779	+6·471
	8	·722	80·931	27·049	26·775	6·225

0^h Gr.		Diameter of Ball. Equat.	Phase prec. *l*.	Polar	Axis of Ring. Major	Minor	*Mimas.* a_1	b_1	l_1-L.	Diff.
1884.		″	″	″	″	″	″	″	°	°
Aug.	31	17·60	0·066	16·21	40·57	18·17	27·68	−12·40	260·79	1909·81
Sept.	5	17·76	·068	16·35	40·93	18·32	27·92	12·50	10·60	·86
	10	17·92	·069	16·50	41·30	18·48	28·17	12·60	120·46	·91
	15	18·08	·070	16·65	41·68	18·64	28·43	12·71	230·37	1909·96
	20	18·25	·070	16·80	42·06	18·80	28·69	12·82	340·33	1910·01
	25	18·42	·069	16·96	42·46	18·96	28·96	12·94	90·34	·05
	30	18·59	·068	17·12	42·85	19·13	29·23	13·05	200·39	·10

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0^h Gr.		Diameter of Ball. Equat.	Phase prec. *l.*	Polar	Axis of Ring. Major	Minor	*Mimas.* a_1	b_1	$l_1-L.$	Diff.
1884.										
Oct.	5	18″·76	″·065	17″·27	43″·24	19″·30	29″·50	−13″·17	310°·49	1910·14
	10	18·93	·061	17·43	43·63	19·47	29·76	13·28	60·63	·19
	15	19·09	·056	17·58	44·01	19·64	30·02	13·40	170·82	·24
	20	19·25	·051	17·73	44·38	19·81	30·27	13·51	281·06	·28
	25	19·41	·045	17·87	44·73	19·97	30·51	13·62	31·34	·32
	30	19·55	·039	18·00	45·07	20·12	30·74	13·72	141·66	·35
Nov.	4	19·68	·032	18·13	45·38	20·27	30·95	−13·82	252·01	1910·37
	9	19·81	·025	18·24	45·66	20·40	31·15	13·92	2·38	·40
	14	19·92	·019	18·34	45·91	20·52	31·32	14·00	112·78	·42
	19	20·01	·013	18·43	46·13	20·63	31·47	14·07	223·20	·43
	24	20·09	·008	18·50	46·31	20·72	31·59	14·13	333·63	·44
	29	20·15	·004	18·55	46·44	20·79	31·68	14·18	84·07	·44
Dec.	4	20·19	·002	18·59	46·53	20·85	31·74	−14·22	194·51	1910·43
	9	20·21	·000	18·61	46·58	20·88	31·78	14·25	304·94	·42
	14	20·21	foll. *l.*	18·61	46·58	20·90	31·78	14·26	55·36	·40
	19	20·19	·001	18·59	46·54	20·89	31·75	14·25	165·76	·38
	24	20·15	·004	18·56	46·45	20·86	31·69	14·23	276·14	·34
	29	20·09	·008	18·51	46·32	20·82	31·60	14·20	26·48	·30
1885.										
Jan.	3	20·02	·013	18·44	46·14	20·75	31·48	−14·15	136·78	1910·25
	8	19·92	·018	18·35	45·93	20·66	31·33	14·10	247·03	·21
	13	19·81	·024	18·25	45·68	20·56	31·16	14·03	357·24	·16
	18	19·69	·031	18·14	45·40	20·45	30·97	13·95	107·40	·10
	23	19·56	·038	18·02	45·09	20·32	30·75	13·86	217·50	1910·04
	28	19·41	·044	17·88	44·75	20·18	30·52	13·77	327·54	1909·98
Feb.	2	19·26	·050	17·74	44·39	20·03	30·28	−13·67	77·52	1909·93
	7	19·10	·055	17·59	44·02	19·88	30·03	13·56	187·45	·87
	12	18·93	·060	17·44	43·64	19·72	29·77	13·45	297·32	·81
	17	18·76	·064	17·29	43·25	19·56	29·50	13·34	47·13	·75
	22	18·59	·067	17·13	42·85	19·39	29·23	13·23	156·88	·69
	27	18·42	·068	16·97	42·46	19·22	28·96	13·11	266·57	·64

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0^h Gr.	Diameter of Ball. Equat.	Phase prec. l	Polar.	Axis of Ring. Major	Minor	*Mimas.* a_1	b_1	l_1-L.	Diff.
1884.	″	″	″	″	″	″	″	°	°
Mar. 4	18·25	·069	16·81	42·06	19·06	28·69	−13·00	16·21	1909·58
9	18·08	·069	16·66	41·68	18·89	28·43	12·89	125·79	·54
14	17·91	·068	16·51	41·30	18·73	28·17	12·78	235·33	·49
19	17·75	·066	16·36	40·93	18·58	27·92	12·67	344·82	·45
24	17·60	·063	16·22	40·57	18·42	27·67	12·57	94·27	·41
29	17·45	·059	16·08	40·22	18·28	27·44	12·47	203·68	·37
Apr. 3	17·31	·055	15·95	39·89	18·14	27·21	−12·37	313·05	1909·34
8	17·17	0·051	15·82	39·58	18·00	27·00	12·28	62·39	

	Enceladus. a_2	b_2	l_2-L	Diff.	*Tethys.* a_3	b_3	l_3-L	Diff.
1884.	″	″	°	°	″	″	°	°
Aug. 31	35·51	−15·90	111·28	1313·46	43·95	−19·69	115·36	953·26
Sept. 5	35·82	16·03	344·74	·50	44·34	19·85	348·62	·30
10	36·14	16·17	218·24	·56	44·74	20·02	221·92	·35
15	36·47	16·31	91·80	·60	45·15	20·19	95·27	·40
20	36·81	16·45	325·40	·65	45·57	20·36	328·67	·45
25	37·15	16·59	199·05	·70	45·99	20·54	202·12	·49
30	37·50	16·74	72·75	·75	46·42	20·73	75·61	·55
Oct. 5	37·84	−16·89	306·50	1313·80	46·84	−20·91	309·16	953·59
10	38·18	17·04	180·30	·84	47·26	21·09	182·75	·65
15	38·51	17·19	54·14	·89	47·68	21·28	56·40	·69
20	38·84	17·33	288·03	·93	48·08	21·46	290·09	·74
25	39·15	17·47	161·96	1313·97	48·46	21·63	163·83	·78
30	39·44	17·61	35·93	1314·01	48·82	21·80	37·61	·82
Nov. 4	39·71	−17·73	269·94	1314·05	49·16	−21·95	271·43	953·85
9	39·96	17·85	143·99	·07	49·47	22·10	145·28	·88
14	40·18	17·96	18·06	·09	49·74	22·23	19·16	·91
19	40·37	18·05	252·15	·11	49·97	22·35	253·07	·93
24	40·52	18·13	126·26	·13	50·16	22·45	127·00	·95
29	40·64	18·20	0·39	·13	50·31	22·53	0·95	·95
Dec. 4	40·72	−18·24	234·52	1314·13	50·41	−22·59	234·90	953·96
9	40·76	18·27	108·65	·12	50·46	22·62	108·86	·95
14	40·77	18·29	342·77	·11	50·46	22·64	342·81	·94
19	40·73	18·28	216·88	·08	50·42	22·63	216·75	·92
24	40·65	18·26	90·96	·06	50·32	22·60	90·67	·90
29	40·53	18·22	325·02	1314·02	50·17	22·55	324·57	·87

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		Enceladus.				*Tethys.*			
		a_2	b_2	l_2-L	Diff.	a_3	b_3	l_3-L	Diff.
1884.		″	″	°	°	″	″	°	°
Jan.	3	40·38	−18·16	199·04		49·98	−22·48	198·44	
					1313·99				953·83
	8	40·19	18·08	73·03		49·75	22·39	72·27	
					·94				·79
	13	39·97	18·00	306·97		49·48	22·28	306·06	
					·89				·75
	18	39·72	17·90	180·86		49·17	22·15	179·81	
					·84				·70
	23	39·45	17·78	54·70		48·84	22·01	53·51	
					·79				·64
	28	39·16	17·66	288·49		48·48	21·86	287·15	
					·73				·59
Feb.	2	38·85	−17·53	162·22		48·09	−21·70	160·74	
					1313·68				953·54
	7	38·52	17·40	35·90		47·69	21·53	34·28	
					·62				·48
	12	38·19	17·26	269·52		47·27	21·36	267·76	
					·56				·43
	17	37·85	17·11	143·08		46·85	21·18	141·19	
					·50				·37
	22	37·50	16·97	16·58		46·42	21·00	14·56	
					·45				·31
	27	37·15	16·82	250·03		45·99	20·82	247·87	
					·40				·26
Mar.	4	36·81	−16·68	123·43		45·57	−20·64	121·13	
					1313·35				953·21
	9	36·47	16·53	356·78		45·15	20·47	354·34	
					·29				·16
	14	36·14	16·39	230·07		44·73	20·29	227·50	
					·25				·12
	19	35·81	16·26	103·32		44·33	20·12	100·62	
					·21				·07
	24	35·50	16·12	336·53		43·95	19·96	333·69	
					·16				953·02
	29	35·20	15·99	209·69		43·57	19·80	206·71	
					·13				952·99
Apr.	3	34·91	−15·87	82·82		43·22	−19·65	79·70	
					1313·09				952·96
	8	34·64	15·75	315·91		42·88	19·50	312·66	

		Dione.				*Rhea.*			
		a_4	b_4	l_4-L	Diff.	a_5	b_5	l_5-L	Diff.
1884.		″	″	°	°	″	″	°	°
Aug.	31	56·29	−25·21	65·46		78·61	−35·21	149·39	
					657·41				398·16
Sept.	5	56·79	25·42	2·87		79·30	35·50	187·55	
					·46				·21
	10	57·30	25·63	300·33		80·02	35·80	225·76	
					·51				·26
	15	57·82	25·85	237·84		80·75	36·11	264·02	
					·55				·31
	20	58·36	26·08	175·39		81·50	36·42	302·33	
					·61				·36
	25	58·90	26·31	113·00		82·26	36·74	340·69	
					·65				·40
	30	59·45	26·54	50·65		83·02	37·07	19·09	
					·71				·45
Oct.	5	59·99	−26·78	348·36		83·78	−37·40	57·54	
					657·75				398·51
	10	60·53	27·02	286·11		84·53	37·73	96·05	
					·81				·56
	15	61·06	27·25	223·92		85·27	38·06	134·61	
					·85				·60
	20	61·57	27·48	161·77		85·99	38·38	173·21	
					·90				·65
	25	62·06	27·70	99·67		86·67	38·69	211·86	
					·94				·70
	30	62·53	27·92	37·61		87·32	38·98	250·56	
					657·98				·75

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		Dione.				*Rhea.*			
		a_4	b_4	l_4-L	Diff.	a_2	b_2	l_2-L	Diff.
1884.		"	"	°	°	"	"	°	°
Nov.	4	62·96	−28·12	335·59	658·02	87·92	−39·26	289·31	398·78
	9	63·35	28·30	273·61	·05	88·47	39·52	328·09	·81
	14	63·70	28·47	211·66	·09	88·96	39·76	6·90	·85
	19	64·00	28·62	149·75	·10	89·38	39·97	45·75	·87
	24	64·25	28·75	87·85	·12	89·72	40·15	84·62	·89
	29	64·43	28·85	25·97	·14	89·98	40·29	123·51	·91
Dec.	4	64·56	−28·93	324·11	658·14	90·16	−40·40	162·42	398·91
	9	64·63	28·97	262·25	·13	90·25	40·46	201·33	·91
	14	64·63	28·99	200·38	·13	90·26	40·49	240·24	·90
	19	64·57	28·98	138·51	·11	90·17	40·48	279·14	·90
	24	64·45	28·95	76·62	·09	90·00	40·42	318·04	·87
	29	64·26	28 88	14·71	·06	89·74	40·33	356·91	·85
1885.									
Jan.	3	64·02	−28·79	312·77	658·03	89·40	−40·20	35·76	398·81
	8	63·72	28·67	250·80	657·99	88·99	40·04	74·57	·78
	13	63·37	28·53	188·79	·95	88·50	39·85	113·35	·74
	18	62·98	28·37	126·74	·90	87·95	39·62	152·09	·69
	23	62·55	28·19	64·64	·85	87·35	39·37	190·78	·65
	28	62·09	28·00	2·49	·80	86·70	39·10	229·43	·59
Feb.	2	61·59	−27 80	300·29	657·75	86·01	−38·82	268·02	398·54
	7	61·08	27·58	238·04	·69	85·30	38·52	306·56	·49
	12	60·55	27·36	175·73	·64	84·55	38·21	345·05	·44
	17	60·00	27·13	113·37	·58	83·80	37·89	23·49	·38
	22	59·46	26·90	50·95	·52	83·03	37·57	61·87	·33
	27	58·91	26·67	348·47	·48	82·26	37·24	100·20	·27
Mar.	4	58·36	−26·44	285·95	657·42	81·50	−36·92	138·47	398·22
	9	57·82	26·21	223·37	·38	80·75	36·61	176·69	·18
	14	57·29	25·99	160·75	·32	80·01	36·30	214·87	·13
	19	56·78	25·77	98·07	·28	79·29	35·99	253·00	·08
	24	56·28	25·56	35·35	·24	78·60	35·70	291·08	·03
	29	55·81	25·36	332·59	·20	77·93	35·41	329·11	398·00
Apr.	3	55·35	−25·16	269·79	657·17	77·30	−35·14	7·11	397·96
	8	54·92	24·98	206·96		76·69	34·88	45·07	

Approximate Greenwich Mean Times of conjunctions of the satellites with the centre of *Saturn*:

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"n" inferior conjunction with centre, or satellite exactly in the direction of the minor axis of the ring, north, moving from the following to the preceding side.

"s" superior conjunction, or satellite south, moving from the preceding to the following side.

1884.	h		
Aug. 31	8·1	Te.	n.
	9·2	Rh.	n.
	20·9	Di.	n.
Sept. 1	6·8	Te.	s.
2	5·5	Te.	n.
	5·8	Di.	s.
	15·5	Rh.	s.
3	4·1	Te.	s.
	14·6	Di.	n.
4	2·8	Te.	n.
	21·7	Rh.	n.
	23·5	Di.	s.
5	1·4	Te.	s.
6	0·1	Te.	n.
	8·3	Di.	n.
	22·7	Te.	s.
7	4·0	Rh.	s.
	17·2	Di.	s.
	21·4	Te.	n.
8	20·1	Te.	s.
9	2·0	Di.	n.
	10·2	Rh.	n.
	18·7	Te.	n.
10	10·9	Di.	s.
	17·4	Te.	s.
11	16·0	Te.	n.
	16·4	Rh.	s.
	19·7	Di.	n.
12	14·7	Te.	s.
13	4·6	Di.	s.
	13·3	Te.	n.
	22·7	Rh.	n.
14	12·0	Te.	s.
	13·4	Di.	n.
	15·6	En.	s.

1884.	h		
Sept. 15	10·7	Te.	n.
	22·3	Di.	s.
16	4·9	Rh.	s.
	9·3	Te.	s.
	16·9	En.	n.
17	7·1	Di.	n.
	8·0	Te.	n.
18	6·6	Te.	s.
	11·1	Rh.	n.
	15·3	Mi.	n.
	16·0	Di.	s.
	18·3	En.	s.
19	5·3	Te.	n.
	10·7	En.	n.
	13·9	Mi.	n.
20	0·8	Di.	n.
	3·9	Te.	s.
	12·5	Mi.	n.
	17·4	Rh.	s.
	19·6	En.	n.
21	2·6	Te.	n.
	9·7	Di.	s.
	11·2	Mi.	n.
	12·0	En.	s.
22	1·2	Te.	s.
	18·5	Di.	n.
	20·9	En.	s.
	21·1	Mi.	s.
	23·6	Rh.	n.
	23·9	Te.	n.
23	13·4	En.	n.
	19·7	Mi.	s.
	22·6	Te.	s.
24	3·4	Di.	s.
	18·3	Mi.	s.

1884.	h		
Sept. 24	21·2	Te.	n.
	22·3	En.	n.
25	5·8	Rh.	s.
	12·2	Di.	n.
	14·7	En.	s.
	16·9	Mi.	s.
	19·9	Te.	s.
26	15·6	Mi.	s.
	18·5	Te.	n.
	21·1	Di.	s.
27	12·0	Rh.	n.
	14·2	Mi.	s.
	16·0	En.	n.
	17·2	Te.	s.
28	5·9	Di.	n.
	8·5	En.	s.
	12·8	Mi.	s.
	15·8	Te.	n.
29	11·4	Mi.	s.
	14·5	Te.	s.
	14.8	Di.	s.
	17·4	En.	s.
	18·2	Rh.	s.
30	9·8	En.	n.
	10·0	Mi.	s.
	13·1	Te.	n.
	23·6	Di.	n.
Oct. 1	11·8	Te.	s.
	18·7	En.	n.
	19·9	Mi.	n.
2	0·5	Rh.	n.
	8·4	Di.	s.
	10·4	Te.	n.
	11·1	En.	s.
	18·6	Mi.	n.

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1884.	h		
Oct.	3	9·1	Te. s.
		17·2	Mi. n.
		17·3	Di. n.
		20·0	En. s.
	4	6·7	Rh. s.
		7·7	Te. n.
		12·4	En. n.
		15·8	Mi. n.
	5	2·1	Di. s.
		6·4	Te. s.
		14·4	Mi. n.
		21·3	En. n.
	6	5·0	Te. n.
		11·0	Di. n.
		12·9	Rh. n.
		13·0	Mi. n.
		13·8	En. s.
	7	3·7	Te. s.
		19.8	Di. s.
	8	2·3	Te. n.
		19·1	Rh. s.
	9	1·0	Te. s.
		4·6	Di. n.
		23·6	Te. n.
	10	13·5	Di. s.
		22·3	Te. s.
	11	1·3	Rh. n.
		8·8	En. n.
		17·4	Mi. s.
		20·9	Te. n.
		22·3	Di. n.
	12	16·0	Mi. s.
		17·7	En. n.
		19·6	Te. s.
	13	7·1	Di. s.
		7·5	Rh. s.
		10·2	En. s.
		14·6	Mi. s.
		18·2	Te. n.
	14	13·3	Mi. s.

1884.	h		
Oct.	14	16·0	Di. n.
		16·9	Te. s.
		19·1	En. s.
	15	11·5	En. n.
		11·9	Mi. s
		13·7	Rh. n.
		15·5	Te. n.
	16	0·8	Di. s.
		10·5	Mi. s.
		14·2	Te. s.
		20·4	En. n.
	17	9·7	Di. n.
		12·8	En. s.
		12·8	Te. n.
		19·9	Rh. s.
		20·4	Mi. n.
	18	11·5	Te. s.
		18·5	Di. s.
		19·0	Mi. n.
		21·7	En. s.
	19	10·1	Te. n.
		14·1	En. n.
		17·6	Mi. n.
	20	2·0	Rh. n.
		3·3	Di. n.
		6·6	En. s.
		8·8	Te. s.
		16·3	Mi. n.
	21	7·4	Te. n.
		12·2	Di. s.
		14·9	Mi. n.
		15·4	En. s.
	22	6·1	Te. s.
		7·9	En. n.
		8·2	Rh. s.
		13·5	Mi. n.
		21·0	Di. n.
	23	4·7	Te. n.
		12·1	Mi. n.
		16·8	En. n.

1884.	h		
Oct.	24	3·4	Te. s.
		5.8	Di. s.
		9·2	En. s.
		10·7	Mi. n.
		14·4	Rh. n.
	25	2·0	Te. n.
		9·3	Mi. n.
		14·6	Di. n.
		18·1	En. s.
	26	0·7	Te. s.
		10·5	En. n.
		19·3	Mi. s.
		20·6	Rh. s.
		23·3	Te. n.
		23·5	Di. s.
	27	17·9	Mi. s.
		19·4	En. n.
		22·0	Te. s.
	28	8·3	Di. n.
		11·8	En. s.
		16·5	Mi. s.
		20·6	Te. n.
	29	2·8	Rh. n.
		15·1	Mi. s.
		17·1	Di. s.
		19·3	Te. s.
		20·7	En. s.
	30	13·2	En. n.
		13·7	Mi. s.
		17·9	Te. n.
	31	2·0	Di. n.
		5·6	En. s.
		8·9	Rh. s.
		12·3	Mi. s.
		16·6	Te. s.
Nov.	1	10·8	Di. s.
		10·9	Mi. s.
		14·5	En. s.
		15·2	Te. n.
	2	6·9	En. n.

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1884.	h		
Nov. 2	9·6	Mi.	s.
	13·8	Te.	s.
	15·1	Rh.	n.
	19·6	Di.	n.
3	12·5	Te.	n.
	15·8	En.	n.
4	4·4	Di.	s.
	11·1	Te.	s.
	21·3	Rh.	s.
5	9·8	Te.	n.
	13·3	Di.	n.
6	8·4	Te.	s.
	22·1	Di.	s.
7	3·4	Rh.	n.
	7·1	Te.	n.
	13·9	Mi.	n.
	18·4	En.	n.
8	5·7	Te.	s.
	6·9	Di.	n.
	10·8	En.	s.
	12·5	Mi.	n.
9	4·4	Te.	n.
	9·6	Rh.	s.
	11·2	Mi.	n.
	15·7	Di.	s.
	19·7	En.	s.
10	3·0	Te.	s.
	9·8	Mi.	n.
	12·2	En.	n.
11	0·6	Di.	n.
	1·7	Te.	n.
	4·6	En.	s.
	8·4	Mi.	n.
	15·8	Rh.	n.
	19·7	Mi.	s.
	21·0	En.	n.
12	0·3	Te.	s.
	9·4	Di.	s.
	13·5	En.	s.
	18·3	Mi.	s.

1884.	h		
Nov. 12	22·9	Te.	n.
13	5·9	En.	n.
	16·9	Mi.	s.
	18·2	Di.	n.
	21·6	Te.	s.
	21·9	Rh.	s.
14	14·8	En.	n.
	15·5	Mi.	s.
	20·2	Te.	n.
15	3·0	Di.	s.
	7·2	En.	s.
	14·1	Mi.	s.
	18·9	Te.	s.
16	4·1	Rh.	n.
	11·9	Di.	n.
	12·7	Mi.	s.
	16·1	En.	s.
	17·5	Te.	n.
17	8·5	En.	n.
	11·4	Mi.	s.
	16·2	Te.	s.
	20·7	Di.	s.
18	10·0	Mi.	s.
	10·2	Rh.	s.
	14·8	Te.	n.
	17·4	En.	n.
19	5·5	Di.	n.
	8·6	Mi.	s.
	9·8	En.	s.
	13·4	Te.	s.
	19·9	Mi.	n.
20	12·1	Te.	n.
	14·3	Di.	s.
	16·4	Rh.	n.
	18·5	Mi.	n.
	18·7	En.	s.
21	10·7	Te.	s.
	11·2	En.	n.
	17·1	Mi.	n.
	23·2	Di.	n.

1884.	h		
Nov. 22	9·4	Te.	n.
	15·7	Mi.	n.
	20·0	En.	n.
	22·5	Rh.	s.
23	8·0	Di.	s.
	8·0	Te.	s.
	12·5	En.	s.
	14·3	Mi.	n.
24	4·9	En.	n.
	6·7	Te.	n.
	13·0	Mi.	n.
	16·8	Di.	n.
	21·3	En.	s.
25	4·7	Rh.	n.
	5·3	Te.	s.
	11·6	Mi.	n.
	13·8	En.	n.
26	1·6	Di.	s.
	3·9	Te.	n.
	6·2	En.	s.
	10·2	Mi.	n.
27	2·6	Te.	s.
	8·8	Mi.	n.
	10·4	Di.	n.
	10·8	Rh.	s.
	15·1	En.	s.
28	1·2	Te.	n.
	7·4	Mi.	n.
	7·5	En.	n.
	18·7	Mi.	s.
	19·3	Di.	s.
	23·9	Te.	s.
29	6·0	Mi.	n.
	16·4	En.	n.
	17·0	Rh.	n.
	17·3	Mi.	s.
	22·5	Te.	n.
30	4·1	Di.	n.
	8·8	En.	s.
	15·9	Mi.	s.

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1884.	h			1884.	h			1884.	h		
Nov. 30	21·2	Te.	s.	Dec. 10	23·7	Rh.	s.	Dec. 20	0·3	Rh.	s.
Dec. 1	12·9	Di.	s.	11	2·6	Di.	n.		5·5	En.	n.
	19·8	Te.	n.		6·2	Te.	n.		10·8	Mi.	s.
	23·1	Rh.	s.		7·8	En.	s.		16·4	Di.	s.
2	18·4	Te.	s.		12·0	Mi.	n.		16·7	Te.	n.
	21·7	Di.	n.	12	4·9	Te.	s.	21	9·4	Mi.	s.
3	17·1	Te.	n.		10·6	Mi.	n.		14·4	En.	n.
4	5·3	Rh.	n.		11·5	Di.	s.		15·3	Te.	s.
	6·5	Di.	s.		16·7	En.	s.	22	1·2	Di.	n.
	10·4	Mi.	s.	13	3·5	Te.	n.		6·5	Rh.	n.
	11·5	En.	s.		5·9	Rh.	n.		6·8	En.	s.
	15·7	Te.	s.		9·1	En.	n.		8·0	Mi.	s.
5	0·0	Mi.	s.		9·2	Mi.	n.		13·9	Te.	n.
	14·4	Te.	n.		20·3	Di.	n.	23	6·6	Mi.	s.
	15·4	Di.	n.	14	2·2	Te.	s.		10·0	Di.	s.
	20·3	Mi.	n.		7·8	Mi.	n.		12·6	Te.	s.
	20·3	En.	s.		18·0	En.	n.		15·6	En.	s.
6	7·6	Mi.	s.	15	0·8	Te.	n.		18·0	Mi.	n.
	11·4	Rh.	s.		5·1	Di.	s.	24	8·1	En.	n.
	12·8	En.	n.		6·4	Mi.	n.		11·2	Te.	n.
	13·0	Te.	s.		10·4	En.	s.		12·6	Rh.	s.
	18·9	Mi.	n.		12·0	Rh.	s.		16·6	Mi.	n.
7	0·2	Di.	s.		17·7	Mi.	s.		18·8	Di.	n.
	5·2	En.	s.		23·4	Te.	s.	25	9·9	Te.	s.
	6·2	Mi.	s.	16	13·9	Di.	n.		15·2	Mi.	n.
	11·7	Te.	n.		16·4	Mi.	s.		17·0	En.	n.
	17·5	Mi.	n.		19·3	En.	s.	26	3·7	Di.	s.
	21·6	En.	n.		22·1	Te.	n.		8·5	Te.	n.
8	9·0	Di.	n.	17	11·8	En.	n.		9·4	En.	s.
	10·3	Te.	s.		15·0	Mi.	s.		13·8	Mi.	n.
	14·1	En.	s.		18·2	Rh.	n.		18·8	Rh.	n.
	16·1	Mi.	n.		20·7	Te.	s.	27	1·9	En.	n.
	17·6	Rh.	n.		22·7	Di.	s.		7·2	Te.	s.
9	6·5	En.	n.	18	13·6	Mi.	s.		12·4	Mi.	n.
	8·9	Te.	n.		19·4	Te.	n.		12·5	Di.	n.
	14·8	Mi.	n.		20·6	En.	n.		18·3	En.	s.
	17·8	Di.	s.	19	7·6	Di.	n.	28	5·8	Te.	n.
10	7·6	Te.	s.		12·2	Mi.	s.		10·8	En.	n.
	13·4	Mi.	n.		13·1	En.	s.		11·3	Mi.	n.
	15·4	En.	n.		18·0	Te.	s.		21·3	Di.	s.

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1884.	h		
Dec. 29	0·9	Rh.	s.
	4·5	Te.	s.
30	3·1	Te.	n.
	6·1	Di.	n.
31	1·7	Te.	s.
	7·1	Rh.	n.
	15·0	Di.	s.
1885.			
Jan. 1	0·4	Te.	n.
	13·4	En.	n.
	16·8	Mi.	s.
	23·0	Te.	s.
	23·8	Di.	n.
2	5·8	En.	s.
	13·2	Rh.	s.
	15·4	Mi.	s.
	21·7	Te.	n.
3	8·6	Di.	s.
	14·0	Mi.	s.
	14·7	En.	s.
	20·3	Te.	s.
4	7·1	En.	n.
	12·6	Mi.	s.
	17·4	Di.	n.
	19·0	Te.	n.
	19·4	Rh.	n.
5	11·2	Mi.	s.
	16·0	En.	n.
	17·6	Te.	s.
6	2·3	Di.	s.
	8·4	En.	s.
	9·9	Mi.	s.
	16·3	Te.	n.
7	0·9	En.	n.
	1·6	Rh.	s.
	8·5	Mi.	s.
	11·1	Di.	n.
	14·9	Te.	s.
	17·3	En.	s.
8	7·1	Mi.	s.

1885.	h		
Jan. 8	9·8	En.	n.
	13·5	Te.	n.
	18·4	Mi.	n.
	19·9	Di.	s.
9	2·2	En.	s.
	5·7	Mi.	s.
	7·7	Rh.	n.
	12·2	Te.	s.
	17·0	Mi.	n.
	18·6	En.	n.
10	4·7	Di.	n.
	10·8	Te.	n.
	11·1	En.	s.
	15·6	Mi.	n.
11	3·5	En.	n.
	9·5	Te.	s.
	13·6	Di.	s.
	13·9	Rh.	s.
	14·2	Mi.	n.
12	8·1	Te.	n.
	12·4	En.	n.
	12·9	Mi.	n.
	22·4	Di.	n.
13	4·8	En.	s.
	6·8	Te.	s.
	11·5	Mi.	n.
	20·1	Rh.	n.
14	5.4	Te.	n.
	7·2	Di.	s.
	10·1	Mi.	n.
	13·7	En.	s.
15	4·1	Te.	s.
	6·2	En.	n.
	8·7	Mi.	n.
	16·1	Di.	n.
16	2·2	Rh.	s.
	2·7	Te.	n.
	7·3	Mi.	n.
	15·0	En.	n.
17	0·9	Di.	s.

1885.	h		
Jan. 17	1·4	Te.	s.
	5·9	Mi.	n.
	7·5	En.	s.
18	0·0	Te.	n.
	4·6	Mi.	n.
	8·4	Rh.	n.
	9·7	Di.	n.
	15·9	Mi.	n.
	16·4	En.	s.
	22·7	Te.	s.
19	8·8	En.	n.
	14·5	Mi.	s.
	18·5	Di.	s.
	21·3	Te.	n.
20	13·1	Mi.	s.
	14·6	Rh.	s.
	17·7	En.	n.
	20·0	Te.	s.
21	3·4	Di.	n.
	10·1	En.	s.
	11·7	Mi.	s.
	18·6	Te.	n.
22	2·6	En.	n.
	10·3	Mi.	s.
	12·2	Di.	s.
	17·3	Te.	s.
	19·0	En.	s.
	20·8	Rh.	n.
23	8·9	Mi.	s.
	11·4	En.	n.
	15·9	Te.	n.
	21.0	Di.	n.
24	3·9	En.	s.
	7·6	Mi.	s.
	14·6	Te.	s.
25	2·9	Rh.	s.
	5·9	Di.	s.
	13·2	Te.	n.
26	11·9	Te.	s.
	14·7	Di.	n.

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1885.	h			1885.	h			1885.	h		
Jan. 27	9·1	Rh.	n.	Feb. 5	9·9	Rh.	n.	Feb. 14	12·5	Mi.	n.
	10·5	Te.	n.		11·8	En.	s.		18·5	Di.	n.
	23·5	Di.	s.		13·6	Mi.	s.	15	2·0	En.	s.
28	6·5	En.	s.		21·0	Te.	n.		7·6	Te.	n.
	9·2	Te.	s.	6	4·3	En.	n.		11·1	Mi.	n.
	13 3	Mi.	n.		12·2	Mi.	s.	16	3·3	Di.	s.
29	7·8	Te.	n.		13·4	Di.	n.		6·2	Te.	s.
	8·4	Di.	n.		19·7	Te.	s.		9·7	Mi.	n.
	12·0	Mi.	n.	7	10·8	Mi.	s.		10·9	En.	s.
	15·3	Rh.	s.		13·2	En.	n.		16·9	Rh.	s.
	15·4	En.	s.		16·1	Rh.	s.	17	3·4	En.	n.
30	6·5	Te.	s.		18·3	Te.	n.		4·9	Te.	n.
	7·9	En.	n.		22·2	Di.	s.		8·3	Mi.	n.
	10·6	Mi.	n.	8	5·6	En.	s.		12·2	Di.	n.
	17·2	Di.	s.		9·5	Mi.	s.	18	3·5	Te.	s.
31	0·3	En.	s.		17·0	Te.	s.		7·0	Mi.	n.
	5·1	Te.	n.	9	7·1	Di.	n.		12·3	En.	n.
	9·0	Mi.	n.		8·1	Mi.	s.		21·0	Di.	s.
	16.7	En.	n.		14·5	En.	s.		23·1	Rh.	n.
	21·5	Rh.	s.		15·6	Te.	n.	19	2·2	Te.	n.
Feb. 1	2·1	Di.	n.		22·3	Rh.	n.		4·7	En.	s.
	3·8	Te.	s.	10	6·7	Mi.	s.		5·6	Mi.	n.
	7·8	Mi.	n.		6·9	En.	n.	20	0·8	Te.	s.
	9·2	En.	s.		14·3	Te.	s.		4·2	Mi.	n.
2	1·6	En.	n.		15·9	Di.	s.		5·8	Di.	n.
	2·4	Te.	n.	11	5·3	Mi.	s.		13·6	En.	s.
	6·4	Mi.	n.		12·9	Te.	n.		23·5	Te.	n.
	10·9	Di.	s.		15·8	En.	n.	21	5·4	Rh.	s.
3	1·1	Te.	s.	12	0·8	Di.	n.		14·7	Di.	s.
	3·7	Rh.	s.		4·5	Rh.	s.		22·2	Te.	s.
	5·1	Mi.	n.		8·3	En.	s.	22	20·8	Te.	n.
	10·5	En.	n.		11·6	Te.	s.		23·5	Di.	n.
	16·4	Mi.	s.		15·2	Mi.	n.	23	11·6	Rh.	n.
	19·7	Di.	n.	13	0·7	En.	n.		19·5	Te.	s.
	23·7	Te.	n.		9·6	Di.	s.	24	8·4	Di.	s.
4	2·9	En.	s.		10·3	Te.	n.		10·0	Mi.	s.
	15·0	Mi.	s.		13·9	Mi.	n.		16·3	En.	s.
	19·4	En.	n.	14	8·9	Te.	s.		18·1	Te.	n.
	22·4	Te.	s.		9·6	En.	n.	25	8·6	Mi.	s.
5	4·6	Di.	s.		10·7	Rh.	n.		8·7	En.	n.

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1885.	h		
Feb. 25	16·8	Te.	s.
	17·2	Di.	n.
	17·8	Rh.	s.
26	1·2	En.	s.
	7·2	Mi.	s.
	15·5	Te.	n.
	17·6	En.	n.
27	2·1	Di.	s.
	5·9	Mi.	s.
	10·0	En.	s.
	14·1	Te.	s.
28	0·0	Rh.	n.
	2·5	En.	n.
	4·5	Mi.	s.
	10·9	Di.	n.
	12.8	Te.	n.
Mar. 1	11·4	En.	n.
	11·4	Te.	s.
	14·4	Mi.	n.
	19·8	Di.	s.
2	3·8	En.	s.
	6·3	Rh.	s.
	10·1	Te.	n.
	13·0	Mi.	n.
3	4·7	Di.	n.
	8·7	Te.	s.
	11·7	Mi.	n.
	12·7	En.	s.
4	5·2	En.	n.
	7·4	Te.	n.
	10·3	Mi.	n.
	12·5	Rh.	n.
	13·5	Di.	s.
5	6·1	Te.	s.
	8·9	Mi.	n.
	14·1	En.	n.
	22·4	Di.	n.
6	4·7	Te.	n.
	6·5	En.	s.
	7·5	Mi.	n.

1885.	h		
Mar. 6	18·8	Rh.	s.
7	3·4	Te.	s.
	6·2	Mi.	n.
	7·2	Di.	s.
	15·4	En.	s.
8	2·0	Te.	n.
	4·8	Mi.	n.
	7·8	En.	n.
	16·1	Di.	n.
	16·1	Mi.	s.
9	0·3	En.	s.
	0·7	Te.	s.
	1·0	Rh.	n.
	3·4	Mi.	n.
	23·4	Te.	n.
10	0·9	Di.	s.
	9·2	En.	s.
	13·3	Mi.	s.
	22·0	Te.	s.
11	1·6	En.	n.
	7·2	Rh.	s.
	9·8	Di.	n.
	12·0	Mi.	s.
	20·7	Te.	n.
12	10·5	En.	n.
	10·6	Mi.	s.
	18·6	Di.	s.
	19·4	Te.	s.
13	3·0	En.	s.
	9·2	Mi.	s.
	13·5	Rh.	n.
	18·0	Te.	n.
14	3·5	Di.	n.
	7·8	Mi.	s.
	11·9	En.	s.
	16·7	Te.	s.
15	4·3	En.	n.
	6·5	Mi.	s.
	12·4	Di.	s.
	15·3	Te.	n.

1885.	h		
Mar. 15	19·7	Rh.	s.
16	5·1	Mi.	s.
	13·2	En.	n.
	14·0	Te.	s.
	21·2	Di.	n.
17	3.7	Mi.	s.
	5·7	En.	s.
	12·7	Te.	n.
18	2·0	Rh.	n.
	6·1	Di.	s.
	11·3	Te.	s.
	13·6	Mi.	n.
	14·6	En.	s.
19	7·0	En.	n.
	10·0	Te.	n.
	12·3	Mi.	n.
	15·0	Di.	n.
20	8·2	Rh.	s.
	8·7	Te.	s.
	10·9	Mi.	n.
	15·9	En.	n.
	23·8	Di.	s.
21	7·3	Te.	n.
	8·3	En.	s.
	9·5	Mi.	n.
22	6·0	Te.	s.
	8·7	Di.	n.
	14·5	Rh.	n.
23	4·6	Te.	n.
	17·5	Di.	s.
24	3·3	Te.	n.
	20·8	Rh.	s.
25	2·0	Te.	n.
	2·4	Di.	n.
26	0·6	Te.	s.
	11·3	Di.	s.
	23·3	Te.	n.
27	3·8	Rh.	n.
	20·1	Di.	n.
	22·8	Te.	s.

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1885.	h			1885.	h			1885.	h		
Mar. 28	20·6	Te.	n.	Mar. 31	22·7	Di.	s.	April 5	1·3	Di.	n.
29	5·0	Di.	s.	April 1	15·3	Te.	n.		4·1	Rh.	n.
	9·3	Rh.	s.	2	7·6	Di.	n.		10·0	Te.	n.
	19·3	Te.	s.		14·0	Te.	s.	6	8·6	Te.	s.
30	13·9	Di.	n.		21·8	Rh.	s.		10·2	Di.	s.
	18·0	Te.	n.	3	12·6	Te.	n.	7	7·3	Te.	n.
31	15·6	Rh.	n.		16·5	Di.	s.		10·4	Rh.	s.
	16·6	Te.	s.	4	11·3	Te.	s.		19·1	Di.	n.

By means of this list of conjunctions, approximate values of the coordinates x and y, expressed in semi-diameters of the planet's equator, may be easily found for any other time t in the following little table, the argument of which is the interval τ between the time t and the time of the nearest preceding or following conjunction.

τ	Mimas.		Enceladus.		Tethys.		Dione.		Rhea.	
h	x_1	y_1	x_2	y_2	x_3	y_3	x_4	y_4	x_5	y_5
0	0·0	1·4	0·0	1·8	0·0	2·3	0·0	2·9	0·0	4·0
1	0·9	1·4	0·8	1·7	0·7	2·2	0·6	2·9	0·5	4·0
2	1·7	1·2	1·5	1·7	1·4	2·2	1·2	2·8	1·0	4·0
3	2·3	0·9	2·2	1·5	2·0	2·1	1·8	2·8	1·5	4·0
4	2·8	0·6	2·8	1·3	2·6	1·9	2·4	2·7	2·0	3·9
5	3·1	0·3	3·3	1·0	3·2	1·7	2·9	2·6	2·5	3·8
6	3·1	0·1	3·7	0·7	3·7	1·5	3·5	2·4	3·0	3·8
7	2·9	0·5	3·9	0·4	4·1	1·3	4·0	2·3	3·5	3·7
8	2·5	0·9	4·0	0·1	4·5	1·0	4·4	2·1	4·0	3·6
9					4·7	0·7	4·8	1·9	4·5	3·5
10					4·9	0·4	5·2	1·7	4·9	3·4
11					5·0	0·1	5·6	1·4	5·3	3·2
12							5·8	1·2	5·7	3·1
13							6·1	0·9	6·1	2·9
14							6·2	0·7	6·5	2·8
15							6·3	0·4	6·8	2·6
16							6·4	0·1	7·1	2·4
20									8·2	1·6
27·1									8·9	0·0

The satellites move in the direction of *de*creasing position-angles. They are at their greatest elongations *Mi.* $5^h{\cdot}7$, *En.* $8^h{\cdot}2$ *Te.* $11^h{\cdot}3$, *Di.* $16^h{\cdot}4$, *Rh.* $27^h{\cdot}1$, before or after the times of conjunction. Observers who are desirous to follow the motions of the satellites will do well to lay down the data of the table graphically on a sufficiently large scale, so that, by marking the

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corresponding times for the night of observing, they may get information about the places of the satellites at a glance.

The following ephemerides of the three outer satellites give their positions in reference to the circle of declination. I publish the ephemeris of *Hyperion* not without hesitation, as the Washington observations made during the last three oppositions have not yet become known to me (the last series which I have seen is that of 1880, published in No. 2394 of the *Astron. Nachrichten*), and the available data are not sufficient for such a determination of the orbit as to allow the preparation of accurate predictions four years in advance. Nevertheless I prefer to run the risk of giving a detailed ephemeris, in order, if possible, to induce the possessors of powerful instruments to take part in the observations of *Hyperion* and to contribute accurate data for the proper investigation of the problem of the motion of the satellite.

Differences of Right Ascension and Declination between the Satellites Titan, Hyperion, and Iapetus, and the Centre of Saturn.

	Titan.		*Hyperion.*		*Iapetus.*	
0^h Gr. 1884.	α_1-A	δ_1-D	α_7-A	δ_7-D	α_8-A	δ_8-D
	s	"	s	"	s	'
Aug. 31	+11·64	+ 47·3	+10·45	− 72·6	+28·80	+174·0
Sept. 1	+ 8·41	+ 68·6	+13·67	− 52·5	+26·85	+175·7
2	+ 3·82	+ 78·9	+16·03	− 28·9	+24·74	+176·5
3	− 1·42	+ 76·2	+17·41	− 3·5	+22·48	+176·3
4	− 6·43	+ 60·9	+17·71	+ 22·2	+20·07	+175·0
5	−10·40	+ 35·3	+16·90	+ 46·7	+17·54	+172·7
6	−12·70	+ 3·9	+15·00	+ 68·1	+14·90	+169·3
7	−12·98	− 28·2	+12·06	+ 85·0	+12·16	+164·9
8	−11·28	− 56·2	+ 8·19	+ 95·5	+ 9·33	+159·6
9	− 7·89	− 75·9	+ 3·66	+ 98·2	+ 6·43	+153·3
10	− 3·36	− 84·8	− 1·21	+ 92·1	+ 3·49	+146·0
11	+ 1·68	− 81·6	− 5·98	+ 76·5	+ 0·51	+137·8
12	+ 6·50	− 67·0	−10·10	+ 52·4	− 2·49	+128·7
13	+10·42	− 42·8	−13·00	+ 21·9	− 5·48	+118·8
14	+12·85	− 12·4	−14·30	− 11·5	− 8·45	+108·1
15	+13·40	+ 19·9	−13·82	− 43·5	−11·38	+ 96·7
16	+11·91	+ 49·3	−11·68	− 70·5	−14·25	+ 84·7
17	+ 8·58	+ 70·9	− 8·26	− 89·8	−17·04	+ 72·1
18	+ 3·83	+ 81·2	− 3·99	−100·4	−19·73	+ 58·9
19	− 1·57	+ 78·1	+ 0·65	−102·0	−22·30	+ 45·3
20	− 6·75	+ 61·9	+ 5·27	− 95·4	−24·74	+ 31·4
21	−10·80	+ 35·4	+ 9·51	− 81·7	−27·02	+ 17·2
22	−13·11	+ 3·0	+13·13	− 62·4	−29·14	+ 2·9
23	−13·35	− 29·6	+15·91	− 38·9	−31·07	− 11·6
24	−11·54	− 58·6	+17·71	− 13·0	−32·79	− 25·9

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		Titan.		*Hyperion.*		*Iapetus.*	
0^h Gr. 1884.		α_1-A	δ_1-D	α_7-A	δ_7-D	α_8-A	δ_8-D
		s	″	s	″	s	″
Sept.	25	− 8·01	− 78·6	+18·41	+ 13·9	−34·30	− 40·1
	26	− 3·30	− 87·3	+17·97	+ 40·0	−35·59	− 54·2
	27	+ 1·90	− 83·6	+16·38	+ 63·6	−36·64	− 67·9
	28	+ 6·84	− 68·1	+13·62	+ 82·9	−37·44	− 81·2
	29	+10·84	− 42·9	+ 9·88	+ 96·2	−37·99	− 94·0
	30	+13·29	− 11·5	+ 5·33	+101·7	−38·28	−106·2
Oct.	1	+13·78	+ 21·8	+ 0·29	+ 98·2	−38·31	−117·7
	2	+12·17	+ 51·8	− 4·79	+ 84·9	−38·08	−128·4
	3	+ 8·63	+ 73·7	− 9·35	+ 62·2	−37·59	−138·3
	4	+ 3·69	+ 83·7	−12·83	+ 31·8	−36·84	−147·3
	5	− 1·88	+ 79·9	−14·68	− 2·2	−35·83	−155·4
	6	− 7·17	+ 62·6	−14·71	− 36·3	−34·57	−162·4
	7	−11·28	+ 34·9	−12·96	− 66·2	−33·07	−168·3
	8	−13·56	+ 1·3	− 9·75	− 88·8	−31·35	−173·1
	9	−13·70	− 32·6	− 5·51	−102·4	−29·40	−176·8
	10	−11·73	− 61·6	− 0·75	−106·7	−27·25	−179·3
	11	− 8·01	− 81·6	+ 4·09	−102·2	−24·91	−180·6
	12	− 3·11	− 89·9	+ 8·65	− 89·9	−22·39	−180·7
	13	+ 2·25	− 85·4	+12·62	− 71·3	−19·72	−179·6
	14	+ 7·31	− 68·8	+15·79	− 48·0	−16·90	−177·4
	15	+11·35	− 42·4	+17·96	− 21·6	−13·96	−174·0
	16	+13·76	− 9·7	+19·02	+ 6·3	−10·92	−169·4
	17	+14·13	+ 24·4	+18·90	+ 33·9	− 7·80	−163·7
	18	+12·35	+ 54·9	+17·55	+ 59·3	− 4·61	−156·9
	19	+ 8·59	+ 76·8	+15·01	+ 80·9	− 1·38	−149·0
	20	+ 3·43	+ 86·2	+11·38	+ 96·6	+ 1·87	−140·2
	21	− 2·31	+ 81·3	+ 6·83	+104·6	+ 5·12	−130·5
	22	− 7·69	+ 62·7	+ 1·67	+103·5	+ 8·35	−119·9
	23	−11·81	+ 33·6	− 3·66	+ 92·3	+11·54	−108·5
	24	−14·01	− 1·1	− 8·60	+ 71·1	+14·67	− 96·4
	25	−13·99	− 35·8	−12·54	+ 39·9	+17·71	− 83·7
	26	−11·82	− 65·1	−14·92	+ 6·5	+20·66	− 70·4
	27	− 7·88	− 84·8	−15·42	− 29·3	+23·49	− 56·6
	28	− 2·78	− 92·3	−14·02	− 61·7	+26·19	− 42·4
	29	+ 2·73	− 86·7	−11·01	− 87·1	+28·74	− 27·8
	30	+ 7·87	− 68·8	− 6·83	−103·6	+31·12	− 13·1
	31	+11·90	− 41·0	− 1·99	−107·8	+33·32	+ 1·7
Nov.	1	+14·22	− 7·3	+ 3·05	−107·7	+35·33	+ 16·7

The rest will be published in the Supplementary Number.

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Ephemeris of the Satellite of Neptune, 1884–5. By A. Marth.

P, angle of position of the minor axis of the satellite's apparent orbit, in the direction of superior conjunction.

a, b, major and minor semi-axes of the apparent orbit.

$u-U$, longitude of the satellite in its orbit, reckoned from the point which is in superior conjunction with the planet or in opposition to the Earth.

$U+180°$, planetocentric longitude of the Earth, reckoned in the satellite's orbit from the ascending node on the celestial equator.

B, planetocentric latitude of the Earth above the plane of the satellite's orbit.

0^h Gr.	P	a	b	$u-U$	Diff.	U	B
1884.	°	″	″	°	°	°	°
Sept. 5	320·51	16·61	7·55	222·56	612·53	135·66	27·04
15	320·45	16·70	7·59	115·09	·48	135·75	27·02
25	320·34	16·78	7·61	7·57	·43	135·89	26·97
Oct. 5	320·21	16·85	7·62	260·00	·38	136·07	26·90
15	320·04	16·90	7·62	152·38	·34	136·30	26·81
25	319·86	16·94	7·61	44·72	·30	136·55	26·70
Nov. 4	319·66	16·97	7·59	297·02	·29	136·82	26·59
14	319·45	16·97	7·56	189·31	·28	137·10	26·47
24	319·24	16·96	7·53	81·59	·29	137·39	26·34
Dec. 4	319·05	16·93	7·48	333·88	·29	137·66	26·22
14	318·87	16·89	7·43	226·17	·32	137·90	26·11
24	318·72	16·83	7·38	118·49	·36	138·11	26·01
1885.							
Jan. 3	318·60	16·75	7·33	10·85	·40	138·27	25·93
13	318·51	16·67	7·27	263·25	·45	138·39	25·87
23	318·47	16·58	7·22	155·70	·50	138·45	25·83
Feb. 2	318·46	16·48	7·18	48·20	·56	138·46	25·82
12	318·49	16·39	7·14	300·76	·62	138·41	25·83
22	318·57	16·30	7·11	193·38	·67	138·30	25·86
Mar. 4	318·69	16·21	7·09	86·05	·73	138·13	25·92
14	318·84	16·13	7·07	338·78	612·78	137·92	26·01
24	319·02	16·05	7·07	231·56		137·66	26·11

The values of P, a, b, and $u-U$ are to be interpolated for the times for which the apparent positions of the satellite are required, and the position-angles p and distances s are then found by means of the formulæ

$$s \sin (P-p) = a \sin (u-U)$$
$$s \cos (P-p) = b \cos (u-U).$$

The satellite moves in the direction of decreasing position-angles, and will be at its greatest elongations and at its *superior* and *inferior* conjunctions with the planet at the following hours, G.M.T.:—

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	inf. Elong. $p = P+90^\circ$ $s =$ a		*sup.* Conj. P b		*sup.* Elong. $P-90^\circ$ a		*inf.* Conj. $P+180^\circ$ b	
1884.		h		h		h		h
Sept.	5	18·6	Sept. 7	5·8	Sept. 8	17·1	Sept. 10	4·4
	11	15·6	13	2·9	14	14·2	16	1·4
	17	12·7	19	0·0	20	11·2	21	22·5
	23	9·8	24	21·0	26	8·3	27	19·6
	29	6·8	30	18·1	Oct. 2	5·4	Oct. 3	16·6
Oct.	5	3·9	Oct. 6	15·2	8	2·5	9	13·7
	11	1·0	12	12·3	13	23·6	15	10·8
	16	22·1	18	9·4	19	20·7	21	7·9
	22	19·2	24	6·5	25	17·8	27	5·0
	28	16·3	30	3·6	31	14·9	Nov. 2	2·1
Nov.	3	13·4	Nov.	0·7	Nov 6	12·0	7	23·2
	9	10·5	10	21·8	12	9·1	13	20·4
	15	7·6	16	18·9	18	6·2	19	17·5
	21	4·7	22	16·0	24	3·3	25	14·6
	27	1·8	28	13·1	30	0·4	Dec. 1	11·7
Dec.	2	23·0	Dec. 4	10·2	Dec. 5	21·5	7	8·8
	8	20·1	10	7·3	11	18·6	13	5·9
	14	17·2	16	4·5	17	15·7	19	3·0
	20	14·3	22	1·6	23	12·8	25	0·1
	26	11·4	27	22·7	29	9·9	30	21·2
1885.								
Jan.	1	8·5	Jan. 2	19·7	Jan. 4	7·0	Jan. 5	18·3
	7	5·6	8	16·8	10	4·1	11	15·4
	13	2·6	14	13·9	16	1·2	17	12·5
	18	23·7	20	11·0	21	22·3	23	9·5
	24	20·8	26	8·1	27	19·3	29	6·6
	30	17·8	Feb. 1	5·1	Feb. 2	16·4	Feb. 4	3·6
	5	14·9	7	2·2	8	13·4	10	0·7
	11	11·9	12	23·2	14	10·5	15	21·7
	17	9·0	18	20·2	20	7·5	21	18·8
	23	6·0	24	17·3	26	4·5	27	15·8
	1	3·0	Mar. 2	14·3	Mar. 4	1·5	Mar. 5	12·8
	7	0·1	8	11·3	9	22·6	11	9·8
	12	21·1	14	8·3	15	19·6	17	6·8
	18	18·1	20	5·3	21	16·6	23	3·8

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MONTHLY NOTICES

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Note on a Narrow Belt seen on the Planet Saturn.
By A. C. Ranyard.

While observing the planet *Saturn* on the evening of November 4, 1883, at about 10 P.M., with an 18-inch silver-on-glass Reflector, I noticed a narrow dark belt on the body of the planet; it stretched across the disk, and at moments of good definition could be seen to fade away towards either limb; but I thought that the decrease in intensity was not as marked as in the case of similar belts upon *Jupiter*. The colour of the belt was a dark-blue gray, decidedly different from the reddish-brown of the belts upon *Jupiter*. On this evening it was a striking object, nearly as easily seen as the Cassini division in the ring, though not so dark. I estimated its breadth as about double the breadth of the Cassini division, where it is seen broadest in the ansæ. I did not very accurately estimate its position upon the planet; it was quite clear of the projection of the gauze ring upon the ball, and was possibly 10° or 15° to the south of the equatorial regions. On the evening of the 4th the belt was so easily seen that I showed it to two persons who were quite unused to looking through a telescope, and I have little doubt, from their answers to my questions, that they saw the belt I have described. At about eleven o'clock the sky became cloudy, and I had no further chance of examining the planet till the evening of November 13, at about a quarter past twelve, when I again saw the narrow belt stretching all across the ball. The definition was not as good as on the 4th, and the belt was much more difficult to make out than on the former occasion. I thought it was decidedly less dark. It was still narrow, but nebulous, and there was no comparison between its visibility and the visibility of the Cassini division. I continued observing it till half-past one, and

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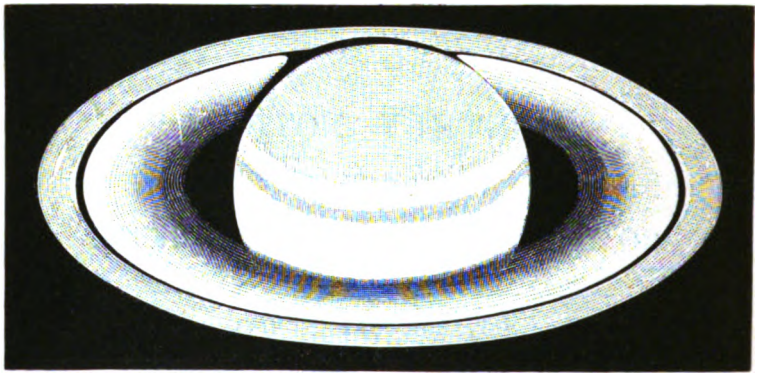
only saw it in the intervals of good definition. However, I had no doubt about its being there, and that it stretched all across the disk.



Saturn at End of 1883 and Beginning of 1884.

I did not again have an opportunity of examining the planet till the evening of the 21st, at about half-past nine, when the definition was fairly good. I distinctly saw the narrow belt; it still appeared to me to be bluish-gray, decidedly darker than the south polar cloud-cap, from which it was separated by an interval of more than its own breadth. It appeared to me to extend all across the disk, and I again noticed that I could trace it to the limb, with but little if any decrease in intensity. The central part of the belt appeared to me to be below (that is, to the north of) the major axis of the rings, while the extremities of the belt were about an equal distance above the major axis. I was struck by the fact that the inner edge of the gauze ring seen projected upon the body of the planet appeared to be more curved than the narrow belt on the body of the planet. On several other occasions I have noticed the great curvature of the edge of the gauze ring as seen projected on the body of the planet. The Rev. T. W. Webb writes me that he has noticed this appearance, and that to his eye it appeared more curved on the preceding than on the following side. A friend, to whom I pointed out the decided curvature of the inner edge of the gauze belt as compared with the curvature of the outer edge of the rings, suggested that the appearance must be due to the shadow of the rings projected on the ball of the planet; but the position of the sun as compared with the plane of the rings shows that the shadow of the rings at the time could not be seen from the earth. I would suggest that the appearance may probably be explained by the fact that the central part of the disk of the planet is brighter than the limb, and that the gauze belt is best seen where it is projected on the fainter background.

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On the evening of November 21 Mr. B. J. Hopkins was with me, and spent some time in examining the planet. He stated that he distinctly saw the dark belt, but he did not agree with me that it could be traced all across the disk. He, however, concurred with me as to its breadth and its position with regard to the major axis of the rings.

Dr. Copeland writes to me, under date 1883 December 1, that he saw on November 6 "a dark belt on the body of *Saturn*, in about 20° south latitude, sharp towards the equator and shaded off towards the pole. Breadth about twice that of the great division in the ring."

Addendum.

Mr. Denning has sent me a highly-finished drawing of *Saturn*, made with a 12-inch Reflector, on January 7, 1884. It clearly shows the dark belt above described separated from the polar cloud-cap by a brighter division of about its own breadth. He has also sent a drawing of the planet made in November 1882, which shows a similar belt which is not separated by a bright division from the polar cloud-cap.

Mr. George Hunt has also sent me a drawing, showing the narrow belt as seen with his 8-inch Cooke Refractor on the evening of January 24. He describes it as a thickening of the line of demarcation between the bright equatorial belt and the polar cloud-cap. The thickness of the dark belt and its position with regard to the major axis of the rings agrees well with the drawing given above.

There were slight inequalities in the thickness of the narrow belt—it appeared to be more sharply defined on its northern than on its southern edge. I should describe it as nebulous, and certainly not broken up into a chain of cloud masses as shown in the drawing of Trouvelot and Pratt. I have not seen any other belts or streaks upon the ball of the planet, or any white patch at the southern pole.

Nor have I at any time seen any sharp line of demarcation between the gauze ring and the bright ring B, as shown in most drawings. The gauze ring appears to me to shade insensibly into the brighter ring. The inner bright ring (B) appears to have a brighter broad band adjacent to the Cassini division, but I do not feel sure that this may not be due to contrast with the dark space. I have not at any time seen any brighter stripe upon the outermost ring (A), as shown in Mr. De La Rue's celebrated drawing of the planet, and in many other subsequent drawings. Instead of a bright stripe towards the outer edge of ring (A), the light of the ring seems to me sensibly to degrade from near the centre of ring (A) to its outer edge.

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Apparent Places of Comet II. (*Ross*), 1883. By John Tebbutt.

On February 12 last I forwarded the apparent places of a small comet discovered by Mr. Ross, of Elstemwick, Victoria, and observed here from January 19 to February 2. As already intimated, my differential comparisons of that comet were made by means of a square bar-micrometer. The instrument is similar to that described in the Introduction to Cooper's *Catalogue of Ecliptic Stars*, with the addition of spider lines to represent the diagonals of the square. Before commencing the series of comet observations the micrometer was so adjusted that a star at an altitude of 40° or upwards would be bisected by one of the spider lines at the moments of its entering and quitting the field of the telescope. This adjustment was not altered during the whole series of observations, and was repeatedly verified. It was found, however, by numerous comparisons of pairs of stars from Stone's Cape Catalogue, where difference of Declination was considerable, that this adjustment was not consistent with an exact orientation of the micrometer, and a constant was therefore deduced from these observations for the proper correction of the comet's Right Ascensions, the corrections in north-polar distance being hardly sensible. The following table exhibits the corrected places of the comet :—

Windsor Mean Time.					Comet's Apparent R.A.			N.P.D.		
1884.	d	h	m	s	h	m	s	°	′	″
Jan.	19	9	37	42	22	54	35·62	131	46	56·3
	19	9	37	42	22	54	35·53	131	46	56·5
	21	9	26	7	23	5	2·19	131	54	59·2
	21	9	31	39	23	5	3·72	131	55	2·7
	22	9	3	4	23	9	44·51	131	57	21·3
	22	9	3	4	23	9	44·66	131	57	19·5
	23	9	22	11	23	14	18·36	131	58	45·3
	23	9	22	11	23	14	18·47	131	58	43·8
	24	8	51	49	23	18	28·64	131	59	15·1
	24	8	51	49	23	18	28·71	131	59	12·6
	25	9	14	35	23	22	33·60	131	59	19·6
	25	9	14	35	23	22	33·79	131	59	18·7
	27	8	50	59	23	29	56·93	131	57	33·4
	27	8	50	59	23	29	57·04	131	57	31·6
	28	8	56	48	23	33	26·84	131	55	54·1
	28	8	56	48	23	33	26·65	131	55	50·9
Feb.	2	9	38	9	23	48	47·64	131	44	19·1

I trust these corrected places of the comet may be published before a definitive orbit is attempted.

Windsor, N. S. Wales:
1884 *May* 31.

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Observations of the Comet of 1812 (*Pons-Brooks*) *at its return to Perihelion in* 1884. By John Tebbutt.

I herewith forward my complete observations of this interesting comet. They were made with a square bar-micrometer on the 4½-inch Equatorial. The comet ceased to be visible to the unassisted eye on February 22. From January 13 to February 9 it presented a stellar nucleus which admitted of precise observation, and from the latter date to March 2 there was an ill-defined condensation which could be observed with tolerable accuracy. After March 2 it was difficult to observe the comet, for it appeared as a round nebula with scarcely any condensation in its centre. On April 1 it was excessively faint in consequence of the increasing moonlight. After the withdrawal of the full moon the comet was found again on April 21, but it was of the last degree of faintness, and its position could therefore be only roughly determined as follows:—

April 21 $7^h\ 27^m\ 14^s$ Windsor M.T. App. R.A. $= 4^h\ 17^m\ 53^s{\cdot}8$.

App. N.P.D. $155^\circ\ 45'\ 37''$.

In determining the places of the comparison stars proper motion has been applied where practicable. The parallax corrections are based on Schulhof and Bossert's Ephemerides in Nos. 2558, 2569, and 2570 of the *Astronomische Nachrichten*, with an assumed equatorial horizontal solar parallax of $8''{\cdot}85$.

Apparent Places of the Comet of 1812 (Pons-Brooks).

Windsor M. T.				App. R.A.			App. N.P.D.			Parallax Corrections. R.A. +	Parallax Corrections. N.P.D. +	No. of Comps.	Comp. Star.
1884.	d	h	m	s	h	m	s	°	′	″	s	″		
Jan.	13	8	25	55	23	3	46·24	88	20	32·6	0·73	7·7	7	1
	19	8	19	13	23	37	33·25	99	14	9·2	0·68	6·4	5	2
	19	8	24	8	*	+5	21·84	*	−1	35·0	0·69	6·5	6	3
	21	8	30	14	23	47	37·43	102	35	24·3	0·68	6·0	6	4
	21	8	30	14	23	47	37·30	102	35	19·4	0·68	6·0	6	5
	22	8	6	50	23	52	18·26	104	9	53·4	0·65	5·6	5	6
	23	8	17	24	23	56	58·69	105	43	42·2	0·65	5·4	3	7
	23	8	17	24	23	56	58·78	105	43	44·9	0·65	5·4	3	8
	25	8	32	16	0	5	49·45	108	42	6·6	0·66	5·2	8	9
Feb.	2	8	22	40	*	−1	41·27	*	+15	42·7	0·61	3·5	5	10
	2	8	22	40	0	35	28·30	118	37	1·6	0·61	3·5		11
	2	8	22	40	0	35	28·39	118	36	58·8	0·61	3·5	5	12
	4	8	24	37	0	41	42·29	120	40	3·8	0·61	3·3	14	13

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Windsor M. T.				App. R.A.			App. N.P.D.			Parallax Corrections. R.A. +	N.P.D. +	No. of Comps.	Comp. Star.
1884. d	h	m	s	h	m	s	°	′	″	s	″		
5	8	34	54	0	44	40·81	121	38	35·7	0·62	3·3	5	14
5	8	34	54	0	44	40·62	121	38	32·0	0·62	3·3	5	15
5	8	34	54	0	44	40·54	121	38	50·8	0·62	3·3	5	16
8	8	33	13	0	52	59·38	124	20	16·5	0·61	3·0	10	17
9	8	23	28	0	55	35·17	125	10	2·6	0·60	2·7	3	18
9	8	23	28	0	55	34·95	125	10	3·0	0·60	2·7	3	19
10	8	22	9	0	58	6·37	125	58	18·4	0·60	2·6	10	20
17	8	53	48	1	14	12·69	130	54	34·1	0·61	2·8	5	21
17	8	53	48	1	14	12·15	130	54	35·0	0·61	2·8	5	22
22	8	16	26	1	24	24·46	133	47	58·6	0·59	1·9	10	23
22	8	16	26	1	24	23·97	133	47	56·7	0·59	1·9	10	24
22	8	58	42	1	24	28·11	133	48	56·4	0·61	2·8	5	23
25	8	36	50	1	30	20·26	135	22	3·1	0·60	2·3	4	25
25	8	36	50	1	30	19·96	135	22	2·6	0·60	2·3	4	26
25	8	36	50	1	30	19·73	135	22	0·4	0·60	2·3	4	27
25	8	36	50	1	30	19·72	135	22	2·2	0·60	2·3	4	28
26	8	25	18	1	32	15·74	135	51	27·6	0·60	2·0	8	29
29	8	41	41	1	38	6·24	137	16	49·7	0·60	2·3	4	30
Mar. 2	8	4	26	1	41	56·04	138	9	50·0	0·59	1·6	3	31
2	8	4	26	*	−5	40·18	*	+4	33·6	0·59	1·6	3	32
7	8	42	43	1	51	53·84	140	17	9·5	0·61	2·3	1	33
8	7	37	4	1	53	49·86	140	39	52·4	0·58	1·0	1	33
8	7	37	4	1	53	49·69	140	39	50·9	0·58	1·0	1	34
9	8	2	39	1	55	55·38	141	4	0·6	0·60	1·6	4	35
9	8	2	39	1	55	55·61	141	4	3·9	0·60	1·6	4	36
9	8	2	39	1	55	55·26	141	3	59·9	0·60	1·6	4	37
9	8	59	13	1	56	0·71	141	4	55·3	0·61	2·7	7	37
14	8	7	51	*	−1	56·14	*	−3	10·8	0·62	1·7	6	38
14	8	7	51	2	6	35·94	142	58	52·8	0·62	1·7	6	39
21	8	3	59	2	23	0·92	145	30	57·0	0·64	1·6	3	40
21	8	18	13	2	23	3·82	145	31	5·3	0·64	1·9	7	41
24	8	2	21	2	30	48·05	146	34	18·6	0·65	1·6	2	42
24	8	2	21	2	30	46·99	146	34	17·0	0·65	1·6	2	43
24	8	2	21	2	30	47·20	146	34	15·3	0·65	1·6	2	44
April 1	8	25	25	2	54	14·21	149	20	10·9	0·68	2·0	8	45
1	8	25	25	2	54	13·94	149	20	9·7	0·68	2·0	8	46

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Mean Places of the Comparison Stars for 1884·0, *and the Reductions to the Apparent Places for the Dates of Observation.* By John Tebbutt.

Star.	Mean R.A.	Reduction.	Mean N.P.D.	Reduction.	Authority for Mean Place.
	h m s	s	° ′ ″	″	
1	23 2 44·38	−0·10	88 30 12·4	− 3·6	Lamont 9097; Radcl. Obs. 1858, 1491; Wash. Cat. 1860, 10184; Greenw. 7 Yr. Cat. 1864, 2646; Bruxelles Obs. 1873, 3080 (Lamont N.P.D. rejected).
2	23 35 37·44	−0·11	99 27 53·4	+ 1·1	Schj. 9783.
3	23 32 12	−0·12	99 16	+ 0·9	Approx. Position per Equat. Star 6½ mag.
4	23 41 17·30	−0·14	102 33 8·5	+ 2·2	Schj. 9832, 9833; Greenw. 9 Yr. Cat. 1872. 2224; Bruxelles Obs. 1873, 4109; Cape Cat. 1880, 12286.
5	23 46 49·53	−0·11	102 39 37·2	+ 2·5	Schj. 9890.
6	23 56 16·50	−0·09	104 3 16·0	+ 3·3	Lalande 47123.
7	23 56 51·12	−0·12	105 41 7·0	+ 3·8	Arg. Oeltzen, 23201.
8	0 2 21·07	−0·09	105 28 11·3	+ 4·0	„ 6.
9	0 6 14·96	−0·14	108 34 55·0	+ 5·0	„ 47; Wash. Cat. 1860, 54.
10	0 37 10	−0·27	118 21	+ 8·8	Approx. Position per Equat. Star 8½ mag.
11	0 39 28·75	−0·26	118 24 11·9	+ 9·0	A. O. 395; Cord. Zone 95, 13 and 96, 18.
12	0 50 17·87	−0·20	118 24 15·5	+ 9·6	A. O. 516 and 517; Wash. Cat. 1860, 486; Cord. Zone 97, 11; Cape Cat. 1880, 365.
13	0 40 58·37	−0·33	120 49 30·4	− 9·4	A. O. 410; Wash. Merid. Tr. Zone 77, 100; Wash. Cat. 1860, 384; Cord. Zone 90, 43; Radcl. Obs. 1880, 18; Cape Cat. 1880, 307 (Arg. Oeltzen N.P.D. rejected).
14	0 40 18·28	−0·36	121 20 39·8	+ 9·4	Cordoba Zone 77, 80.
15	0 45 59·51	−0·34	121 36 58·7	+ 9·8	Wash. Merid. Tr. Zone 87, 46; Cord. Zone 77, 90.

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Star	Mean R.A.	Reduction.	Mean N.P.D.	Reduction.	Authority for Mean Place.
	h m s	s	° ′ ″	″	
16	0 46 17·79	−0·33	121 35 39·5	+ 9·9	Wash. Merid. Tr. Zone 87, 47; Cord. Zone 77, 91 (N.P.D. unsatisfactory; the authorities differ by 28″·2).
17	0 59 4·24	−0·38	124 9 17·4	+ 10·9	Wash. Merid. Tr. Zone 100, 19; Wash. Cat. 1860, 555; Cord. Zone 49, 53; Cape Cat. 1880, 418 (Wash. M. Tr. Zone N.P.D. rejected).
18	0 53 55·34	−0·45	125 15 48·5	+ 10·7	Wash. Mural Cir. Zone 83, 16; Wash. Cat. 1860, 512; Cord. Zone 101, 5; Cape Cat. 1880, 384 (W. Mural Cir. Zone R.A. rejected).
19	1 0 50·90	−0·41	125 25 7·9	+ 11·2	Cord. Zone 101, 13; Cape Cat. 1880, 429.
20	1 1 0·15	−0·45	126 16 52·3	+ 11·2	Wash. Mural Cir. Zone 80, 2; Wash. Cat. 1860, 577; Wash Obs. 1873, 45; Cord. Zone 101, 12; Cape Cat. 1880, 431. The individual results for N.P.D. inconsistent.
21	1 0 1·50	−0·71	130 53 28·6	+ 10·9	Cord. Zone 85 91; Cape Cat. 1880, 425.
22	1 13 10·45	−0·64	130 42 52·0	+ 11·9	,, 85, 107.
23	1 23 19·66	−0·80	133 54 45·8	+ 12·2	Melb. Cat. 1870, 97; Cord. Zone 104, 29; Cape Cat. 1880, 580
24	1 24 24·60	−0·79	133 45 13·2	+ 12·3	Cord. Zone 104, 31.
25	1 19 39·60	−0·92	135 7 57·6	+ 11·5	,, 38, 67 and 104, 27; Cape Cat. 1880, 554.
26	1 27 47·08	−0·89	135 20 6·6	+ 12·2	,, 38, 79.
27	1 31 47·16	−0·88	135 23 51·6	+ 12·6	,, 38, 83.
28	1 33 48·87	−0·85	135 11 8·9	+ 12·7	,, 38, 85; Cape Cat. 1880, 653.
29	1 26 43·20	−0·95	136 10 24 1	+ 12·0	Melb. Obs. 1874, 32; Cord Zone 38, 77; Cape Cat. 1880, 606.
30	1 24 35·83	−1·07	137 21 25·1	+ 11·3	Cape Cat. 1880, 587.
31	1 45 40·63	−1·07	138 23 36·5	+ 12·8	Cord. Zone 16, 63; Cape Cat. 1880, 732.

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Star.	Mean R.A.	Reduction.	Mean N.P.D.	Reduction.	Authority for Mean Place.
	h m s	s	° ′ ″	″	
32	1 47 37	−1·04	138 5	+13·0	Approx. Position per Equat. Star 8½ mag.
33	1 51 41·22	−1·24	140 24 16·3	+12·4	Cord. Zone 9, 56 and 83, 156; Cape Cat. 1880, 769.
33	1 51 41·22	−1·26	140 24 16·3	+12·1	„ „ „
34	1 57 31·84	−1·25	140 41 17·3	+12·7	Cord. Zone 9, 60; 83, 164 and 116, 4.
35	1 44 20·26	−1·36	141 3 39·6	+11·3	„ 83, 144; Cape Cat. 1880, 722.
36	1 46 24·34	−1·33	140 46 50·6	+11·4	„ 83, 148; Melb. Obs. 1875,35; Cape Cat. 1880,738.
37	1 57 54·02	−1·29	141 3 7·8	+12·5	„ 83, 165.
38	2 8 34	−1·47	143 2	+12·4	Approx. Position per Equat. Star 9 mag.
39	2 11 37·33	−1·44	142 44 53·1	+12·7	Cord. Zone 119, 17; Cape Cat. 1880, 907.
40	2 35 20·56	−1·66	145 20 12·4	+13·3	Cord. Zone 128, 13; Cape Cat. 1880, 1079. (The N.P. distances of Nos. 1078 and 1079 of the Cape Cat. 1880 have obviously been interchanged. The N.P.D. of 1078 adopted.)
41	2 15 50·80	−1·78	145 32 56·1	+11·3	Cord. Zone 98, 116.
42	2 16 8·74	−1·92	146 28 39·1	+10·5	„ 98, 119; Cape Cat. 1880, 936.
43	2 37 54·57	−1·81	146 28 21·9	+12·8	„ 128, 16.
44	2 38 10·02	−1·84	146 47 29·5	+12·8	„ 128, 18; Cape Cat. 1880, 1108.
45	2 54 40·06	−2·20	149 22 9·2	+12·3	„ 91, 13.
46	3 0 36·73	−2·17	149 23 6·2	+12·9	„ 91, 22; Cape Cat. 1880, 1271.

Windsor, N. S. Wales:
1884 *July* 15.

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Ephemeris for Physical Observations

Greenwich Noon.		Angle of Position of ♃'s Axis.	Latitude of Earth above ♃'s Equator.	Latitude of Sun above ♃'s Equator.	Δ−L.	L−O.	Longitude of ♃'s Central Meridian.	Diff.	Corr. for Phase.
1884.		°	°	°	°	°	°	+	°
Oct.	20	22·860	−0·587	−0·327	− 8·979	15 125	92·73	4391·26	+0·35
	25	23·008	·632	·348	9·368	15·905	163·99	1·33	·38
	30	23·143	·676	·369	9·710	16·638	235·32	1·39	·41
Nov.	4	23·266	−0 719	−0·390	−10·001	17·321	306·71	1·45	+0·44
	9	23·376	·761	·411	10·239	17·949	18·16	1·52	·46
	14	23·474	·801	·431	10·420	18·519	89·68	1·58	·47
	19	23·559	·840	·452	10·538	19·028	161·26	1·65	·48
	24	23·632	·877	·473	10·590	19·471	232·91	1·73	·49
	29	23·692	·912	·493	10·573	19·844	304·64	1·80	·49
Dec.	4	23·741	−0·945	−0·514	−10·484	20·145	16·44	1·87	+0·48
	9	23·777	0·975	·534	10·320	20·370	88·31	1·94	·46
	14	23·801	1·002	·555	10·078	20·518	160·25	2·01	·44
	19	23·813	1·027	·575	9·758	20·586	232·26	2·08	·41
	24	23 813	1·049	·596	9·355	20·572	304·34	2·14	·38
	29	23·801	1·068	·616	8·871	20·477	16·48	2·20	·34
1885.									
Jan.	3	23 776	−1·084	−0·637	−8 307	20·302	88 68	4392·26	+0·30
	8	23·738	1·096	·657	7·666	20·049	160·94	2·31	·26
	13	23·688	1·104	·677	6·949	19·720	233·25	2·35	·21
	18	23·626	1·109	·698	6·161	19·319	305·60	2·38	·17
	23	23·552	1·110	·718	5·308	18·853	17·98	2·41	·12
	28	23·466	1·107	·738	4·398	18·332	90·39	2·42	·08
Feb.	2	23·371	−1·101	−0·758	−3·440	17·763	162 81	2·42	+0·05
	7	23·267	1·091	·778	2·444	17·153	235·23	2·41	·03
	12	23·155	1·078	·798	1·419	16·515	307 64	2·39	+ ·01
	17	23·036	1 063	·818	−0·378	15 861	20·03	2·35	·00
	22	22·914	1·045	·838	+0·666	15·202	92·38	2·30	·00
	27	22·790	1·025	·858	1·703	14·552	164·68	2·24	−·01
Mar.	4	22·667	−1·003	−0 878	+2·720	13·922	236·92	4392·18	−0·03
	9	22·547	0·981	·898	3·706	13·322	309·10	2·10	·06
	14	22·433	·958	·918	4·651	12·762	21·20	2·02	·09
	19	22·326	·935	·938	5·547	12·252	93·22	1·92	·13
	24	22·230	·912	·957	6·384	11·801	165·14	1·83	·18
	29	22·147	·890	·977	7·156	11·415	236·97	1·74	·22

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of Jupiter, 1884–85. By A. Marth.

Greenwich Noon.	Diameter Equat.	Diameter Polar.	Difference of limbs in A. R.	Difference of limbs in Decl.	Defect of illumination. Equat. preced. limb	Defect of illumination. in A. R.	Defect of illumination. in Decl. north *l.*	*d*	*w*
1884.	″	″	s	″	″	s	″	°	°
Oct. 20	33·57	31·43	2·267	31·76	0·21	0·012	0·03	8·98	268·27
25	33·97	31·80	2·292	32·14	·23	·013	·03	9·37	268·19
30	34·39	32·20	2·319	32·54	·25	·015	·03	9·72	268·12
Nov. 4	34·83	32·62	2·347	32·97	0·26	0·016	0·04	10·01	268 04
9	35·31	33·06	2·376	33·42	·28	·017	·04	10·25	267·97
14	35·80	33·52	2·407	33 89	·30	·018	·04	10·43	267·90
19	36·32	34·01	2·441	34·39	·31	·018	·04	10·55	267·82
24	36·86	34·52	2·476	34·90	·31	·019	·04	10·60	267·74
29	37·42	35·04	2·512	35·43	·32	·019	·04	10·58	267·65
Dec. 4	37·99	35·57	2·550	35·98	0·32	0·019	0·04	10·49	267·56
9	38·58	36·12	2·588	36·53	·31	·019	·04	10·33	267·47
14	39·17	36·67	2·627	37·09	·30	·018	·04	10·09	267·36
19	39·76	37·23	2·667	37·65	·29	·017	·04	9·77	267·25
24	40·35	37·78	2·707	38·21	·27	·016	·04	9·37	267·11
29	40·93	38·32	2 746	38·76	·24	·015	·03	8·88	266·96
1885.									
Jan. 3	41·49	38·85	2·785	39·29	0·22	0·013	0·03	8·32	266·79
8	42·03	39·35	2·822	39·80	·19	·011	·02	7·68	266·57
13	42·53	39·82	2·857	40·27	·16	·009	·02	6·96	266·26
18	42·99	40·25	2·890	40·71	·12	·008	·02	6·17	265·89
23	43·40	40·64	2·920	41·10	·09	·006	·01	5·32	265·44
28	43·76	40·97	2·946	41·43	·06	·004	·01	4·42	264·92
Feb. 2	44·05	41·24	2·968	41·70	0·04	0·002	·00	3·46	264·0
7	44·27	41·45	2·986	41·90	·02	·001		2 47	262·2
12	44·42	41·59	2·999	42·04	·00	·000		1·45	258·5
17	44·48	41·65	3 006	42·10	following limb.		south	0·46	235·3
22	44·47	41·64	3·008	42·08	·00	·000	*l.*	0·70	108·4
27	44·38	41·55	3·005	41·99	·01	·001		1·71	96·0
Mar. 4	44·21	41·40	2·997	41·83	0 03	0·002	0·00	2·72	92·8
9	43·97	41·17	2·983	41·59	·05	·003	·01	3·71	91·34
14	43·66	40·88	2·965	41·29	·07	·004	·01	4·65	90·49
19	43·29	40·53	2·942	40·94	·10	·006	·02	5·55	89 91
24	42·87	40·14	2·915	40·54	·13	·008	·02	6·38	89·50
29	42·40	39·70	2·885	40·09	·17	·010	·02	7·16	89·20

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Greenwich Noon.		Angle of Position of ♃'s Axis.	Latitude of Earth above ♃'s Equator.	Latitude of Sun above ♃'s Equator.	A−L.	L−O.	Longitude of ♃'s Central Meridian.	Diff.	Corr. for Phase.
1884.		°	°	°	°	°	°		°
Apr.	3	22·077	−0·870	−0·977	+7·858	11·098	308·71	4391·63	−0·27
	8	22·023	·851	1·016	8·488	10·853	20·34	1·54	·31
	13	21·985	·834	1·036	9·042	10·684	91·88	1·44	·35
	18	21·964	·819	1·055	9·518	10·593	163·32	1·35	·39
	23	21 960	·806	1·074	9·916	10·580	234·67	1·26	·43
	28	21·973	·796	1·094	10·236	10·644	305·93	1·18	·46
May	3	22·003	−0·789	−1·113	+10·481	10·784	17·11	4391·09	−0·48
	8	22·048	·785	1·132	10 652	10·997	88·20	1·01	·49
	13	22·109	·783	1·151	10·751	11·282	159·21	0·95	·50
	18	22·184	·784	1·170	10·782	11·635	230·16	0·88	·51
	23	22·272	·788	1·189	10·747	12·054	301·04	0·82	·50
	28	22·372	·794	1·208	10·649	12·536	11·86	0·77	·49
June	2	22·482	−0·803	−1·227	+10·492	13·076	82·63	0·72	−0·48
	7	22·602	·815	1·246	10·279	13·672	153·35	0·67	·46
	12	22·729	·829	1·264	10·013	14·321	224·03	0·63	·44
	17	22·863	·846	1·283	9·699	15·019	294·66	0 60	·41
	22	23·002	·865	1·302	9·339	15·762	5·26	0·58	·38
	27	23·145	·886	1·320	8·936	16·547	75·84	0·55	·35
July	2	23·291	−0·910	−1·339	+ 8·494	17 371	146·39	4390·53	−0·31
	7	23·438	0·936	1·357	8·017	18·231	216 92		·28

The angle A−L is the difference of the Jovicentric longitudes of the Sun and the Earth, reckoned in the plane of *Jupiter's* equator, L−O+180° the Jovicentric longitude of the Earth reckoned from O, the point of *Jupiter's* vernal equinox or the point of the descending node of the planet's equator on the orbit.

The assumed daily rate of rotation, on which the "Longitude of ♃'s Central Meridian" depends, is 878°·34, the corresponding period being $9^h\ 50^m\ 20^s{\cdot}25$. This rate represents approximately the motion of the white equatorial spot during the interval from the autumn of 1882 to the spring of 1884. As the great reddish spot has so faded away that its place can only be recognised with difficulty, and the white equatorial spot seems likely to remain a fairly defined feature of *Jupiter's* surface for some time to come, I have thought it better to make the change, and to assume the new First or Zero Meridian near the white spot, so that the predicted times of the passages of the First Meridian across the middle of the disk may serve also for those of the white spot, supposing that the motion of the latter has not

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Greenwich Noon.	Diameter Equat.	Diameter Polar.	Difference of limbs in A. R.	Difference of limbs in Decl.	Defect of illumination. Equat. preced. limb	Defect of illumination. in A. R. limb	Defect of illumination. in Decl. north *l.*	*d*	*w*
1884.	"	"	s	"	"	s	"	°	°
Apr. 3	41·89	39·22	2·852	39·61	0·20	0·012	0·03	7·86	88·95
8	41·35	38·71	2·816	39·10	·23	·014	·03	8·49	88·74
13	40·79	38·19	2·778	38·57	·25	·015	·03	9·04	88·56
18	40·21	37·65	2·739	38·02	·28	·017	·04	9·52	88·41
23	39·62	37·10	2·699	37·46	·30	·018	·04	9·92	88·26
28	39·03	36·55	2·659	36·90	·31	·019	·04	10·24	88·13
May 3	38·45	36·00	2·618	36·35	0·32	0·020	0·04	10·49	88·02
8	37·87	35·45	2·577	35·80	·33	·020	·04	10·66	87·91
13	37·30	34·92	2·537	35·27	·33	·020	·04	10·76	87·81
18	36·74	34·40	2·498	34·75	·32	·020	·04	10·79	87·72
23	36·20	33·90	2·460	34·24	·32	·019	·04	10·76	87·62
28	35·68	33·41	2·423	33·75	·31	·019	·04	10·66	87·53
June 2	35·19	32·95	2·387	33·28	0·29	0·018	0·04	10·50	87·44
7	34·71	32·50	2·352	32·84	·28	·017	·03	10·29	87·35
12	34·26	32·08	2·319	32·41	·26	·016	·03	10·02	87·25
17	33·83	31·68	2·288	32·01	·24	·015	·03	9·71	87·16
22	33·43	31·30	2·258	31·63	·22	·014	·03	9·35	87·05
27	33·05	30·95	2·230	31·28	·20	·012	·03	8·95	86·95
July 2	32·70	30·62	2·203	30·95	0·18	0·011	0·02	8·51	86·84
7	32·37	30·31	2·178	30·65	·16	·009	·02	8·03	86·73

changed meanwhile too considerably. In the Supplementary Number of vol. xlii. I have given a list of the observed passages of the white spot, which had become known to me up to August 1882, together with the corresponding longitudes of the spot in a system, in which the adopted daily rate of rotation is 878°·46. In order to reduce these longitudes to the system adopted in the present ephemeris, it will be sufficient to apply the corrections derived by interpolation from the following values:—

	Corr.		Corr.		Corr.
1880, Sept.	6·0	+ 65·0°	1881, July 3·0	+ 29·0°	1882, Jan. 19·0	+ 5·0°
Oct. 1	62		28	26	Feb. 13	+ 2·0
26	59		Aug. 22	23	Mar. 10	− 1·0
Nov. 20	56		Sept. 16	20	Apr. 4	− 4·0
Dec. 15	53		Oct. 11	17		
1881, Jan. 9	50		Nov. 5	14	July 13	−16·0
Feb. 3	47		30	11	Aug. 7·0	−19·0
	28·0	+ 44·0	Dec. 25·0	+ 8·0		

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It would be premature to enter upon the discussion of the changes of the spot's motion till all the observations of it have become available.

If the "Correction for Phase" is added to the longitude of ♃'s Central Meridian or of the meridian directed to the Earth, the longitude of the meridian is found which bisects the illuminated disk of the planet.

The assumed value of *Jupiter's* equatorial diameter is 37″·60 at the distance 5·20273. The assumed proportion of the polar axis to the equatorial diameter is 0·9363. The defect of illumination of the apparent polar diameter is insensible. The last columns give the values of the auxiliary angles d and w required in the computations for defect of illumination, as explained in vol. xl., p. 490ff.

The following is a list of the Greenwich mean times, when the assumed First Meridian of *Jupiter* passes the middle of the illuminated disk:—

1884.	h	m	1884.	h	m	1884.	h	m	1884.	h	m
Oct. 20	17	7·9	Oct. 30	13	14·0	Nov. 9	9	19·7	Nov. 19	5	25·0
21	2	58·2		23	4·2		19	9·9		15	15·3
	12	48·4	31	8	54·5	10	5	0·2	20	1	5·5
	22	38·7		18	44·7		14	50·4		10	55·7
22	8	29·0	Nov. 1	4	35·0	11	0	40·5		20	45·9
	18	19·2		14	25·2		10	30·8	21	6	36·1
23	4	9·5	2	0	15·4		20	21·1		16	26·3
	13	59·7		10	5·7	12	6	11·3	22	2	16·5
	23	50·0		19	55·9		16	1·5		12	6·7
24	9	40·2	3	5	46·2	13	1	51·7		21	56·9
	19	30·5		15	36·4		11	42·0	23	7	47·1
25	5	20·8	4	1	26·6		21	32·2		17	37·3
	15	11·0		11	16·9	14	7	22·4	24	3	27·6
26	1	1·3		21	7·1		17	12·6		13	17·8
	10	51·5	5	6	57·4	15	3	2·9		23	8·0
	20	41·8		16	47·6		12	53·1	25	8	58·2
27	6	32·0	6	2	37·8		22	43·3		18	48·4
	16	22·3		12	28·1	16	8	33·5	26	4	38·6
28	2	12·5		22	18·3		18	23·7		14	28·8
	12	2·8	7	8	8·5	17	4	14·0	27	0	19·0
	21	53·0		17	58·8		14	4·2		10	9·2
29	7	43·3	8	3	49·0		23	54·4		19	59·4
	17	33·5		13	39·2	18	9	44·6	28	5	49·6
30	3	23·7		23	29·5		19	34·8		15	39·8

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1884.	h	m
Nov. 29	1	30·0
	11	20·1
	21	10·3
30	7	0·5
	16	50·7
Dec. 1	2	40·9
	12	31·1
	22	21·3
2	8	11·5
	18	1·7
3	3	51·9
	13	42·1
	23	32·3
4	9	22·4
	19	12·6
5	5	2·8
	14	53·0
6	0	43·2
	10	33·4
	20	23·6
7	6	13·7
	16	3·9
8	1	54·1
	11	44·3
	21	34·5
9		24·6
	17	14·8
10	3	5·0
	12	55·2
	22	45·4
11	8	35·5
	18	25·7
12	4	15·9
	14	6·1
	23	56·2
13	9	46·4
	19	36·6
14	5	26·7
	15	16·9
15	1	7·1

1884.	h	m
Dec. 15	10	57·2
	20	47·4
16	6	37·6
	16	27·8
17	2	17·9
	12	8·1
	21	58·2
18	7	48·4
	17	38·6
19	3	28·7
	13	18·9
	23	9·1
20	8	59·2
	18	49·4
21	4	39·5
	14	29·7
22	0	19·8
	10	10·0
	20	0·2
23	5	50·3
	15	40·5
24	1	30·6
	11	20·8
	21	10·9
25	7	1·1
	16	51·2
26	2	41·4
	12	31·5
	22	21·8
27	8	11·8
	18	2·0
28	3	52·1
	13	42·3
	23	32·4
29	9	22·6
	19	12·7
30	5	2·9
	14	53·0
31	0	43·1
	10	33·3

1884.	h	m
Dec. 31	20	23·4
1885.		
Jan. 1	6	13·6
	16	3·7
2	1	53·9
	11	44·0
	21	34·1
3	7	24·3
	17	14·4
4	3	4·5
	12	54·7
	22	44·8
5	8	35·0
	18	25·1
6	4	15·2
	14	5·4
	23	55·5
7	9	45·6
	19	35·8
8	5	25·9
	15	16·0
9	1	6·1
	10	56·3
	20	46·4
10	6	36·5
	16	26·7
11	2	16·8
	12	6·9
	21	57·0
12	7	47·2
	17	37·3
13	3	27·4
	13	17·5
	23	7·7
14	8	57·8
	18	47·9
15	4	38·0
	14	28·2
16	0	18·3
	10	8·4

1885.	h	m
Jan. 16	19	58·5
17	5	48·6
	15	38·8
18	1	28·9
	11	19·0
	21	9·1
19	6	59·3
	16	49·4
20	2	39·5
	12	29·6
	22	19·7
21	8	9·9
	18	0·0
22	3	50·1
	13	40·2
	23	30·3
23	9	20·4
	19	10·6
24	5	0·7
	14	50·8
25	0	40·9
	10	31·0
	20	21·1
26	6	11·3
	16	1·4
27	1	51·5
	11	41·6
	21	31·7
28	7	21·8
	17	11·9
29	3	2·0
	12	52·1
	22	42·3
30	8	32·4
	18	22·5
31	4	12·6
	14	2·7
	23	52·8
Feb. 1	9	42·9
	19	33·0

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1885.	h	m
Feb. 2	5	23·2
	15	13·3
3	1	3·4
	10	53·5
	20	43·6
4	6	33·7
	16	23·8
5	2	13·9
	12	4·0
	21	54·1
6	7	44·2
	17	34·4
7	3	24·5
	13	14·6
	23	4·7
8	8	54·8
	18	44·9
9	4	35·0
	14	25·1
10	0	15·2
	10	5·4
	19	55·5
11	5	45·6
	15	35·7
12	1	25·8
	11	15·9
	21	6·0
13	6	56·1
	16	46·3
14	2	36·4
	12	26·5
	22	16·6
15	8	6·7
	17	56·8
16	3	46·9
	13	37·0
	23	27·2
17	9	17·3
	19	7·4
18	4	57·5

1885.	h	m
Feb. 18	14	47·6
19	0	37·7
	10	27·9
	20	18·0
20	6	8·1
	15	58·2
21	1	48·3
	11	38·4
	21	28·6
22	7	18·7
	17	8·8
23	2	58·9
	12	49·0
	22	39·2
24	8	29·3
	18	19·4
25	4	9·5
	13	59·7
	23	49·8
26	9	39·9
	19	30·1
27	5	20·2
	15	10·3
28	1	0·5
	10	50·6
	20	40·7
Mar. 1	6	30·9
	16	21·0
2	2	11·1
	12	1·3
	21	51·4
3	7	41·5
	17	31·7
4	3	21·8
	13	11·9
	23	2·1
5	8	52·2
	18	42·4
6	4	32·5
	14	22·7

1885.	h	m
Mar. 7	0	12·8
	10	2·9
	19	53·1
8	5	43·2
	15	33·4
9	1	23·5
	11	13·7
	21	3·8
10	6	54·0
	16	44·1
11	2	34·3
	12	24·4
	22	14·6
12	8	4·8
	17	54·9
13	3	45·1
	13	35·2
	23	25·4
14	9	15·5
	19	5·7
15	4	55·9
	14	46·0
16	0	36·2
	10	26·4
	20	16·5
17	6	6·7
	15	56·9
18	1	47·0
	11	37·2
	21	27·4
19	7	17·6
	17	7·7
20	2	57·9
	12	48·1
	22	38·3
21	8	28·4
	18	18·6
22	4	8·8
	13	59·0
	23	49·2

1885.	h	m
Mar. 23	9	39·4
	19	29·6
24	5	19·7
	15	9·9
25	1	0·1
	10	50·3
	20	40·5
26	6	30·7
	16	20·9
27	2	11·1
	12	1·3
	21	51·5
28	7	41·7
	17	31·9
29	3	22·1
	13	12·3
	23	2·5
30	8	52·7
	18	42·9
31	4	33·1
	14	23·3
Apr. 1	0	13·5
	10	3·7
	19	53·9
2	5	44·1
	15	34·3
3	1	24·5
	11	14·7
	21	5·0
4	6	55·2
	16	45·4
5	2	35·6
	12	25·8
	22	16·0
6	8	6·3
	17	56·5
7	3	46·7
	13	36·9
	23	27·2
8	9	17·4

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1885.	h	m
Apr. 8	19	7·6
9	4	57·8
	14	48·1
10	0	38·3
	10	28·5
	20	18·8
11	6	9·0
	15	59·2
12	1	49·5
	11	39·7
	21	29·9
13	7	20·2
	17	10·4
14	3	0·6
	12	50·9
	22	41·1
15	8	31·4
	18	21·6
16	4	11·9
	14	2·1
	23	52·4
17	9	42·6
	19	32·9
18	5	23·1
	15	13·4
19	1	3·6
	10	53·9
	20	44·0
20	6	34·3
	16	24·6
21	2	14·9
	12	5·2
	21	55·4
22	7	45·7
	17	35·9
23	3	26·2
	13	16·5
	23	6·7
24	8	57·0
	18	47·2

1885.	h	m
Apr. 25	4	37·5
	14	27·8
26	0	18·1
	10	8·3
	19	58·6
27	5	48·9
	15	39·1
28	1	29·4
	11	19·7
	21	10·0
29	7	0·2
	16	50·5
30	2	40·8
	12	31·1
	22	21·3
May 1	8	11·6
	18	1·9
2	3	52·2
	13	42·4
	23	32·7
3	9	23·0
	19	13·3
4	5	3·6
	14	53·9
5	0	44·2
	10	34·4
	20	24·7
6	6	15·0
	16	5·3
7	1	55·6
	11	45·9
	21	36·2
8	7	26·5
	17	16·8
9	3	7·1
	12	57·4
	22	47·7
10	8	38·0
	18	28·2
11	4	18·5

1885.	h	m
May 11	14	8·8
	23	59·1
12	9	49·4
	19	39·8
13	5	30·1
	15	20·4
14	1	10·7
	11	1·0
	20	51·3
15	6	41·6
	16	31·9
16	2	22·2
	12	12·5
	22	2·8
17	7	53·1
	17	43·4
18	3	33·7
	13	24·0
	23	14·4
19	9	4·7
	18	55·0
20	4	45·3
	14	35·6
21	0	25·9
	10	16·2
	20	6·5
22	5	56·9
	15	47·2
23	1	37·5
	11	27·8
	21	18·1
24	7	8·5
	16	58·8
25	2	49·1
	12	39·4
	22	29·7
26	8	20·1
	18	10·4
27	4	0·7
	13	51·0

1885.	h	m
May 27	23	41·4
28	9	31·7
	19	22·0
29	5	12·3
	15	2·7
30	0	53·0
	10	43·3
	20	33·6
31	6	24·0
	16	14·3
June 1	2	4·6
	11	55·0
	21	45·3
2	7	35·6
	17	25·9
3	3	16·3
	13	6·6
	22	56·9
4	8	47·3
	18	37·6
5	4	27·9
	14	18·3
6	0	8·6
	9	58·9
	19	49·3
7	5	39·6
	15	30·0
8	1	20·3
	11	10·6
	21	1·0
9	6	51·3
	16	41·6
10	2	32·0
	12	22·3
	22	12·7
11	8	3·0
	17	53·3
12	3	43·7
	13	34·0
	23	24·4

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1885.	h	m	1885.	h	m	1885.	h	m	1885.	h	m
June 13	9	14·7	June 19	12	49·9	June 25	6	34·8	July 1	0	19·7
19	5·1		22	40·2		16	25·1		10	10·1	
14	4	55·4	20	8	30·6	26	2	15·5	20	0·4	
14	45·7		18	20·9		12	5·8		2	5	50·8
15	0	36·1	21	4	11·3	21	56·2		15	41·2	
10	26·4		14	1·6		27	7	46·6	3	1	31·5
20	16·8		23	52·0		17	36·9		11	21·9	
16	6	7·1	22	9	42·3	28	3	27·3	21	12·2	
15	57·5		19	32·7		13	17·6		4	7	2·6
17	1	47·8	23	5	23·0	23	8·0		16	52·9	
11	38·2		15	13·4		29	8	58·3	5	2	43·3
21	28·5		24	1	3·7	18	48·7		12	33·7	
18	7	18·9	10	54·1		30	4	39·0	22	24·0	
17	9·2		20	44·4		14	29·4		6	8	14·4
19	2	59·6									

In case the remnant of the great reddish spot retains approximately the rate of motion at which it shifted during the interval from the autumn of 1882 to the spring of 1884, its place may be looked for near the Central Meridian at the times which precede or follow the predicted passages of the First Meridian by the interval indicated in the following table:—

1884,	Nov. 14	1885,	Feb. 12	Mar. 29	May 13	4 h.	before	F.M.
	19	Jan. 3	17	Apr. 3	18	3	,,	,,
	23	7	21	7	22	2	,,	,,
	28	12	26	12	27	1	,,	,,
Oct. 19	Dec. 3	17	Mar. 3	16	31	0	,,	,,
23	7	21	7	21	June 5	1	after	,,
28	12	26	12	26	9	2	,,	,,
Nov. 1	16	30	16	30	14	3	,,	,,
6	21	Feb. 4	21	May 5	19	4	,,	,,
11	25	8	25	9	23	5	,,	,,
15	30	13	29	14	28	6	,,	,,

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Ephemerides of the Satellites of Saturn, 1884–85. By A. Marth.

(Continued from page 438.)

Differences of Right Ascension and Declination between the Satellites Titan, Hyperion, and Iapetus, and the Centre of Saturn.

		Titan.		*Hyperion.*		*Iapetus.*	
0h Gr. 1884.		$\alpha_s - A$ s	$\delta_s - D$ "	$\alpha_s - A$ s	$\delta_s - D$ "	$\alpha_s - A$ s	$\delta_s - D$ "
Nov.	1	+14·22	− 7·3	+ 3·05	−107·7	+35·33	+ 16·7
	2	+14·42	+ 27·7	+ 7·87	− 96·9	37·14	31·6
	3	+12·40	+ 58·4	+12·15	− 79·0	+38·72	+ 46·4
	4	+ 8·40	+ 79·9	+15·61	− 56·1	40·08	61·0
	5	− 3·02	+ 88·4	+18·12	− 29·1	+41·21	+ 75·2
	6	− 2·87	+ 82·2	+19·48	− 0·4	42·10	89·1
	7	− 8·29	+ 62·1	+19·63	+ 28·4	+42·74	+102·5
	8	−12·35	+ 31·6	+18·51	+ 55·4	43·12	115·4
	9	−14·41	− 4·2	+16·15	+ 78·7	+43·25	+127·6
	10	−14·17	− 39·4	+12·62	+ 96·5	43·12	139·1
	11	−11·75	− 68·7	+ 8·09	+106·7	+42·74	+149·8
	12	− 7·58	− 87·8	+ 2·87	+107·7	42·10	159·7
	13	− 2·29	− 94·3	− 2·63	+ 98·4	+41·20	+168·7
	14	+ 3·33	− 87·3	− 7·85	+ 78·7	40·05	176·6
	15	+ 8·48	− 68·0	−12·16	+ 49·7	+38·66	+183·6
	16	+12·46	− 38·8	−14·97	+ 14·6	37·04	189·5
	17	+14·61	− 4·1	−15·88	− 22·4	+35·19	+194·2
	18	+14·58	+ 31·4	−14·82	− 56·8	33·12	197·8
	19	+12·29	+ 62·0	−12·01	− 84·7	+30·85	+200·2
	20	+ 8·05	+ 82·8	− 7·91	−103·5	28·37	201·4
	21	+ 2·48	+ 90·1	− 3·03	−112·5	+25·72	+201·3
	22	− 3·50	+ 82·2	+ 2·13	−111·6	22·91	200·0
	23	− 8·92	+ 60·6	+ 7·14	−102·0	+19·95	+197·4
	24	−12·85	+ 28·7	+11·65	− 85·0	16·85	193·6
	25	−14·69	− 7·9	+15·37	− 62·3	+13·65	+188·5
	26	−14·19	− 43·3	+18·13	− 35·4	10·35	182·2
	27	−11·51	− 72·2	+19·72	− 6·3	+ 6·99	+174·7
	28	− 7·12	− 90·3	+20·07	+ 23·2	3·57	166·1
	29	− 1·68	− 95·4	+19·16	+ 51·3	+ 0·13	+156·4
	30	+ 4·01	− 87·0	+16·96	+ 76·0	− 3·32	+145·6
Dec.	1	+ 9·13	− 66·2	+13·55	+ 95·3	− 6·75	+133·8
	2	+12·95	− 35·7	+ 9·11	+107·3	10·13	121·1
	3	+14·87	− 0·3	+ 3·90	+110·2	−13·45	+107·6

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		Titan.		*Hyperion.*		*Iapetus.*	
0h Gr. 1884.		α_8-A s	δ_8-D "	α_7-A s	δ_7-D "	α_8-A s	δ_8-D "
Dec.	4	+14·57	+ 35·3	− 1·67	+102·8	16·68	93·3
	5	+12·02	+ 65·4	− 7·06	+ 84·6	−19·79	+ 78·3
	6	+ 7·54	+ 85·1	−11·63	+ 56·8	22·77	62·8
	7	+ 1·83	+ 90·7	−14·77	+ 22·0	−25·60	+ 46·8
	8	− 4·19	+ 81·3	−16·04	− 15·5	28·25	30·4
	9	− 9·51	+ 58·1	−15·29	− 51·2	−30·71	+ 13·8
	10	−13·19	+ 25·3	−12·73	− 80·8	32·95	− 2·9
	11	−14·82	− 11·8	− 8·76	−101·7	−34·95	− 19·7
	12	−14·04	− 47·0	− 3·92	−112·5	36·70	36·3
	13	−11·11	− 75·0	+ 1·28	−113·4	−38·20	− 52·7
	14	− 6·53	− 91·9	+ 6·38	−105·2	39·43	68·8
	15	− 0·98	− 95·5	+11·02	− 89·2	−40·37	− 84·4
	16	+ 4·67	− 85·6	+14·90	− 67·1	41·03	99·4
	17	+ 9·68	− 63·4	+17·82	− 40·7	−41·40	−113·7
	18	+13·30	− 32·2	+19·60	− 11·7	41·46	127·2
	19	+14·97	+ 3·6	+20·16	+ 18·1	−41·22	−139·8
	20	+14·39	+ 38·9	+19·43	+ 46·7	40·69	151·5
	21	+11·59	+ 68·1	+17·42	+ 72·1	−39·87	−162·1
	22	+ 6·93	+ 86·3	+14·20	+ 92·5	38·77	171·5
	23	+ 1·15	+ 90·4	+ 9·91	+105·9	−37·39	−179·7
	24	− 4·83	+ 79·3	+ 4·82	+110·5	35·74	186·7
	25	− 9·98	+ 55·0	− 0·71	+105·0	−33·85	−192·3
	26	−13·47	+ 21·4	− 6·14	+ 88·8	31·73	196·6
	27	−14·76	− 15·6	−10·87	+ 62·6	−29·38	−199·6
	28	−13·72	− 50·0	−14·26	+ 29·1	26·83	201·1
	29	−10·58	− 77·0	−15·86	− 8·3	−24·10	−201·2
	30	− 5·87	− 92·4	−15·44	− 44·5	21·21	200·0
	31	− 0·32	− 94·5	−13·18	− 75·3	−18·17	−197·4
1885. Jan.	1	+ 5·27	− 83·2	− 9·43	− 97·8	−15·02	−193·4
	2	+10·10	− 60·1	− 4·74	−110·4	−11·77	−188·2
	3	+13·48	− 28·4	+ 0·37	−113·0	8·45	181·6
	4	+14·88	+ 7·3	+ 5·49	−106·4	− 5·08	−173·9
	5	+14·05	+ 42·0	+10·17	− 91·8	− 1·68	165·0
	6	+11·04	+ 69·8	+14·14	− 70·9	+ 1·72	−155·1
	7	+ 6·28	+ 86·5	+17·17	− 45·4	5·10	144·1
	8	+ 0·50	+ 89·0	+19·12	− 17·1	+ 8·45	−132·2
	9	− 5·33	+ 76·6	+19·87	+ 12·3	11·73	119·5
	10	−10·30	+ 51·4	+19·36	+ 40·8	+14·93	−106·0

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		Titan.		*Hyperion.*		*Iapetus.*	
	0h Gr. 1885.	α_8-A s	δ_8-D "	α_7-A s	δ_7-D "	α_8-A s	δ_8-D "
Jan.	11	−13·52	+ 17·7	+17·59	+ 66·6	18·02	91·9
	12	−14·54	− 18·9	+14·62	+ 87·7	+21·00	− 77·2
	13	−13·28	− 52·4	+10·59	+102·3	23·84	62·0
	14	− 9·98	− 77·9	+ 5·70	+108·6	+26·52	− 46·6
	15	− 5·22	− 91·9	+ 0·33	+105·2	29·03	30·8
	16	+ 0·28	− 92·6	− 5·05	+ 91·3	+31·35	− 14·9
	17	+ 5·73	− 80·2	− 9·85	+ 67·5	33·48	+ 1·1
	18	+10·35	− 56·5	−13·46	+ 35·7	+35·40	+ 17·1
	19	+13·48	− 24·8	−15·36	− 0·5	37·10	32·9
	20	+14·64	+ 10·4	−15·33	− 36 5	+38·57	+ 48·5
	21	+13·60	+ 44·0	−13·44	− 68·0	39·81	63·8
	22	+10·46	+ 70·5	−10·00	− 91·8	+40·81	+ 78·7
	23	+ 5·65	+ 85·8	− 5·58	−106·1	41·56	93·1
	24	− 0·05	+ 86·9	− 0 63	−110·7	+42·07	+106·9
	25	− 5·74	+ 73 5	+ 4·37	−106·0	42·33	120·0
	26	−10·45	+ 47·9	+ 9·02	− 93·3	+42·34	+132·4
	27	−13·42	+ 14·4	+13·04	− 74·1	42·09	144·0
	28	−14·21	− 21·4	+16·18	− 50·1	+41·60	+154·8
	29	−12·77	− 53·7	+18 29	− 23·0	40·87	164·6
	30	− 9·39	− 77·9	+19·26	+ 5·5	+39·90	+173·4
	31	− 4·64	− 90·5	+19·03	+ 33·5	38·71	181·1
Feb.	1	+ 0·75	− 90·1	+17·55	+ 59·3	+37·29	+187 8
	2	+ 6·03	− 77·0	+14·92	+ 80 9	35·66	193·4
	3	+10·44	− 53·0	+11·20	+ 96·7	+33 82	+197·8
	4	+13·34	− 21·6	+ 6·63	+104·8	31·79	201·0
	5	+14·30	+ 12·8	+ 1·50	+103·7	+29·58	+203 0
	6	+13·10	+ 45·2	− 3·75	+ 92·6	27·20	203·8
	7	+ 9·89	+ 70·5	− 8·56	+ 71·6	+24·67	+203·4
	8	+ 5·11	+ 84·4	−12·36	+ 42·3	22 00	201·8
	9	− 0·48	+ 84·4	−14·59	+ 8 2	+19·21	+198·9
	10	− 5·97	+ 70·4	−15·01	− 27·3	16·31	194·9
	11	−10·45	+ 44·7	−13·57	− 59·2	+13·33	+189·7
	12	−13·19	+ 11·7	−10·59	− 84·2	10·28	183·4
	13	−13·81	− 23·1	− 6·50	−100·3	+ 7·17	+175·9
	14	−12·26	− 54·2	− 1·81	−107·0	4·04	167·4
	15	− 8·87	− 77·0	+ 3·02	−104·7	+ 0 89	+157·9
	16	− 4·19	− 88·5	+ 7·61	− 94·3	− 2·25	147·5
	17	+ 1·08	− 87·4	+11·65	− 77·2	− 5·37	+136·2

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		Titan.		*Hyperion.*		*Iapetus.*	
	0^h Gr. 1885.	α_1-A s	δ_1-D "	α_2-A s	δ_2-D "	α_3-A s	δ_3-D "
Feb.	18	+ 6·17	− 73·9	+14·90	− 55·2	8·43	124·0
	19	+10·37	− 50·0	+17·20	− 29·7	−11·43	+111·1
	20	+13·09	− 19·2	+18·42	− 2·4	14·35	97·5
	21	+13·89	+ 14·3	+18·50	+ 24·9	−17·16	+ 83·4
	22	+12·61	+ 45·6	+17·40	+ 50·5	19·85	68·8
	23	+ 9·39	+ 69·3	+15·15	+ 72·7	−22·40	+ 53·8
	24	+ 4·69	+ 82·5	+11·85	+ 89·6	24·79	38·5
	25	− 0·75	+ 81·8	+ 7·65	+ 99·6	−27·02	+ 23·0
	26	− 6·05	+ 67·5	+ 2·82	+101·1	29·05	+ 7·5
	27	−10·29	+ 42·1	− 2·25	+ 93·0	−30·88	− 8·1
	28	−12·90	+ 9·8	− 7·06	+ 75·3	32·50	23·5
Mar.	1	−13·39	− 24·0	−11·03	+ 49·0	−33·89	− 38·7
	2	−11·79	− 54·0	−13·64	+ 16·9	35·06	53·6
	3	− 8·44	− 75·7	−14·53	− 17·3	−35·98	− 68·1
	4	− 3·86	− 86·4	−13·63	− 49·2	36·66	82·1
	5	+ 1·25	− 84·7	−11·16	− 75·4	−37·08	− 95·4
	6	+ 6·17	− 71·1	− 7·50	− 93·4	37·26	108·0
	7	+10·20	− 47·6	− 3·13	−102·6	−37·18	−119·9
	8	+12·77	− 17·4	+ 1·50	−102·8	36·86	130·9
	9	+13·48	+ 15·1	+ 5·99	− 95·1	−36·28	−141·0
	10	+12·17	+ 45·3	+10·05	− 80·6	35·47	150·0
	11	+ 8·99	+ 68·4	+13·42	− 60·7	−34·42	−158·1
	12	+ 4·40	+ 80·5	+15·93	− 37·1	33·14	165·0
	13	− 0·90	+ 79·3	+17·44	− 11·2	−31·65	−170·8
	14	− 6·02	+ 65·0	+17·88	+ 15·2	29·96	175·4
	15	−10·14	+ 40·0	+17·19	+ 40·6	−28·07	−178·9
	16	−12·57	+ 8·5	+15·39	+ 63·3	26·01	181·2
	17	−13·00	− 24·2	+12·53	+ 81·4	−23·78	−182·2
	18	−11·41	− 53·2	+ 8·74	+ 93·4	21·41	182·1
	19	− 8·12	− 74·1	+ 4·25	+ 97·7	−18·92	−180·8
	20	− 3·66	− 84·2	− 0·61	+ 92·9	16·31	178·4
	21	+ 1·30	− 82·3	− 5·39	+ 78·7	−13·60	−174·8
	22	+ 6·07	− 68·9	9·54	+ 55·7	10·82	170·1
	23	+ 9·97	− 45·8	−12·52	+ 26·1	− 7·98	−164·4
	24	+12·44	− 16·4	−13·93	− 6·6	5·11	157·7
	25	+13·10	+ 15·3	−13·62	− 38·4	− 2·21	−150·0
	26	+11·81	+ 44·6	−11·71	− 65·7	+ 0·69	141·5
	27	+ 8·70	+ 66·9	− 8·54	− 85·8	+ 3·58	−132·2

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	Titan.		*Hyperion.*		*Iapetus.*	
0h Gr. 1885.	α_1-A s	δ_1-D "	α_7-A s	δ_7-D "	α_8-A s	δ_8-D "
Mar. 28	+ 4·22	+ 78·5	− 4·55	− 97·5	6·44	122·1
29	− 0·94	+ 77·2	− 0·16	−100·6	+ 9·24	−111·3
30	− 5·91	+ 63·1			11·98	99·9
31	− 9·90	+ 38·6			+14·65	− 88·0
Apr. 1	−12·26	+ 7·9			17·22	75·6
2	−12·71	− 24·0			+19·67	− 62·8
3	−11·11	− 52·2			22·00	49·7
4	− 7·91	− 72·5			+24·20	− 36·4
5	− 3·58	− 82·2			26·26	23·0
6	+ 1·25	− 80·3			+28·16	− 9·5
7	+ 5·89	− 67·2			29·89	+ 4·0
8	+ 9·70	− 44·6			+31·45	+ 17·4

Though, for the sake of uniformity, the places of *Hyperion* are here given to $0^s{\cdot}01$ and $0''{\cdot}1$, like those of *Titan* and *Iapetus*, it is to be understood that they may be considerably in error, and that they are merely intended to be sufficiently near the truth to serve the practical requirements of observers. Under present circumstances, and till an approximately correct theory of the disturbed motion of *Hyperion* can be established, a really accurate ephemeris cannot be given. The general effect of the attraction of *Titan* upon the orbit of *Hyperion* may indeed be considered as sufficiently developed; for the evidence of the published observations indicates that the line of apsides of the orbit of *Hyperion* shifts approximately with the line of conjunction of the two satellites, *Hyperion* being, when in conjunction with *Titan*, near the higher apsis of its orbit, or nearly at its greatest distance from *Saturn*. But a knowledge of the observations made during the last three oppositions, together with those of some years to come, is required to allow more detailed inferences to be drawn. The mean conjunctions of the two satellites occur at intervals of 63·637 days in longitudes differing by $-3°{\cdot}267$, so that the line of conjunction accomplishes a full retrograde revolution in longitude in 19·20 years, or, in reference to the lines of apsides of *Titan*, in 18·71 years, in which period 107 (or sometimes 108) conjunctions take place. The line of apsides of the orbit of *Hyperion* oscillates with regard to the line of conjunction, but for the proper determination of the amplitude of oscillation, the phase of which depends on the position of the apsis of *Titan*, sufficient accurate data are not yet available. To get these data, it is of importance that, in deducing the elements of the orbit for a number of epochs, only observations made during the six or seven weeks midway between two successive conjunctions should be used, since during this time the motion of *Hyperion* is not sensibly affected by *Titan*, and the

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elements may be treated as constant. It is only during the two or three weeks including the conjunction, that the alteration of the orbit of *Hyperion*, and especially the retrograde shifting of the apsis, is effected, and the observations made during this state of transition of the elements ought not (for the present) to be mixed up with those preceding or following, nor these with one another. It is, however, very desirable that the observations near the times of conjunctions should be specially attended to; and it will perhaps be most advantageous to observe the places of *Hyperion* at these times, not in reference to the centre of *Saturn*, but to *Titan* itself. During the period over which the present ephemeris extends the conjunctions of the two satellites occur 1884 Oct. 17, Dec. 20, and 1885 Feb. 21, this last being the 210th conjunction since the discovery of *Hyperion*.

Though the predicted places of *Titan* and *Iapetus* may not be much in error, it would be far more satisfactory if they could be founded upon better than makeshift elements. Observers must be good enough to publish all important series of observations of the satellites, if it is desired that the ephemerides should be up to the mark.

Sextant Observations of Comet Pons-Brooks, made on board the ship "British Envoy." By Capt. John P. Holdich, R.N.R.

(*Communicated by Capt. H. Toynbee.*)

Date.	A.T.S. P.M. h	Lat. S. ° ′	Long. E. ° ′	α Tauri. ° ′	α Argus. ° ′	α Orionis. ° ′	α Ceti. ° ′
Jan. 23	9·0	41 40	12 55	74 40	80 14		
26	8·55	44 9	23 50	73 16	85 27		47 0
31	8·55	41 50	44 45	72 28	67 46		
Feb. 3	9·5	42 30	57 45	72 5	64 14		
16	9·10	42 0	98 0		51 34	79 30	
20	9·5	41 20	114 0	73 10	49 12		
	9·0	41 5	118 40	73 20	48 50	78 40	
25	9 10	39 56	133 20	74 18	46 10		

Remarks.

Jan. 26 Better observations than before, comet brighter. Tail about 4° towards α *Ceti*.

Feb. 3 to 16 Moonlight too strong.

16 Tail hardly visible, and nucleus only when Moon is clouded.

20 Fair observations of nucleus. Tail barely visible, except with glasses.

25 Comet getting very indistinct to naked eye, and through glasses getting more the appearance of a nebulous cloud with bright centre.

Sextant observations corrected for index error.
Chronometer time was not taken.
Ship's position by D.R. from noon observations.

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Actuaries, Institute of, Journal, No. cxxix., 8vo. *London*, 1883 — The Institute.

Adelaide, Royal Society of South Australia, Transactions and Proceedings and Report, vols. v. vi., 8vo. *Adelaide*, 1882–83 — The Society.

American Journal of Mathematics, vols. v. 4–vi. 4, 4to. *Baltimore*, 1882–84 — The Editor.

——————— Science, series III., Nos. 150–161, 8vo. *New Haven*, 1883–84 — The Editors.

Analyst, The, Nos. 88–92, 8vo. *London*, 1883 — The Editor.

Anton, F., Bestimmung der Bahn des Planeten (114) *Cassandra*, 8vo. *Wien*, 1883 — The Author.

Association Géodésique Internationale, extrait des Comptes-rendus de la 7me Conférence générale réunie à Rome, 1883 (Unification des longitudes par l'adoption d'un méridien initial unique), 4to. *Berlin.* — The Association.

Astronomical Register, The, Nos. 247–258, 8vo. *London*, 1883–84 — The Editor.

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The Editor. Astronomie (L'), Revue mensuelle d'astronomie populaire, de météorologie et de physique du globe, publiée par Camille Flammarion, années ii. 6–iii. 6, 8vo. *Paris*, 1883–84

——— Astronomische Nachrichten, Nos. 2518–2595, 4to. *Kiel*, 1883–84

——— ——————————— Band 107, 108, 4to. *Kiel*, 1883–84

——— Athenæum, The, Nos. 2902–2954, 4to. *London*, 1883–84

The Author. Ball, L. de, Bestimmung der Constante der täglichen Nutation, 4to. *Kiel*, 1883

The University. Baltimore, John Hopkins University, Circulars, Nos. 24–30, 4to. *Baltimore*, 1883–84

The Society. Batavia, Koninklijke Natuurkundige Vereeniging in Nederlandsch-Indië; Natuurkundig Tijdschrift, deel xlii. (serie VIII., deel iii.), 8vo. *Batavia*, 1883

The Observatory. ——— Observatorium, Regen waarnemingenin Nederlandsch-Indië, 8vo. *Batavia*, 1883

The Author. Beebe, L. H., The dense water of the ocean, its rivers and currents, 8vo.

——— Beilby, J. W., Eureka, an elucidation of mysteries in nature—the problems of Science, 8vo. *Melbourne*, 1883

The Society. Belfast Natural History and Philosophical Society, Report and Proceedings for the Session 1882–83, 8vo. *Belfast*, 1883

The Academy. Berlin, Königliche Akademie der Wissenschaften, Abhandlungen aus dem Jahre 1882, 4to. *Berlin*, 1883

——— ——— Königliche preussische Akademie der Wissenschaften, Sitzungsberichte, 1883, xxii.–1884, xvii., 8vo. *Berlin*, 1883–84

The Institute. ——— Königlich-preussisches geodätisches Institut, Publicationen: Astronomisch-geodätische Arbeiten in den Jahren 1881 und 1882, 4to. *Berlin*, 1883

——— ——————————————————— Gradmessungs-Nivellement zwischen Swinemünde und Amsterdam, bearbeitet von Dr. W. Seibt, 4to. *Berlin*, 1883

The Society. ——— Physikalische Gesellschaft, die Fortschritte der Physik im Jahre 1880, 3 parts, 8vo. *Berlin*, 1882–83

Digitized by Google

Berliner Astronomisches Jahrbuch, 1885–86, 8vo. *Berlin*, 1883–84 — The Observatory, Berlin.

——— Circular, Nos. 207–229, 8vo. *Berlin*, 1883–84 — ———

Birmingham Philosophical Society, Proceedings, vol. iii. 1, 2, 8vo. *Birmingham*, 1882–83 — The Society.

Board of Trade, report on the proceedings and business under the Weights and Measures Act, 1878, fol. *London*, 1883 — H. J. Chaney, Esq.

Boeddicker, O., On the influence of magnetism on the rate of a chronometer, 4to. *Dublin*, 1883 — The Author.

Bologna, Accademia delle Scienze dell' Istituto, Memorie, serie IV. tomo iii., 4to. *Bologna*, 1881 — The Academy.

Bordeaux, Société des sciences physiques et naturelles, Mémoires, série II. tome v. 2, 8vo. *Bordeaux*, 1883 — The Society.

Boston, American Academy of Arts and Sciences, Proceedings, New Series, vol. x., 8vo. *Boston*, 1833 — The Academy.

Bredichin, Th., Histoire de l'hypothèse des ondes cosmiques, composée pour l'explication des formes cométaires, 8vo. *Moscou*, 1883 — The Author.

——— Supplément, 8vo. *Moscou*, 1883 — ———

——— Sur quelques anomalies apparentes dans la structure des queues cométaires, 8vo. *Moscou*, 1883 — ———

——— Sur quelques anomalies apparentes dans la structure des queues cométaires. Sur les anomalies apparentes dans la structure de la grande comète de 1744, 8vo. *Moscou*, 1884 — ———

——— Quelques remarques concernant mes recherches sur les comètes, 8vo. *Moscou*, 1884 — ———

Bristol Museum and Library, Report of proceedings at the 13th annual meeting, 8vo. *Bristol*, 1884 — The Library.

British Association for the Advancement of Science, Report of the 53rd meeting, held at Southport in September 1883, 8vo. *London*, 1884 — The Association.

Bruxelles, Académie Royale des Sciences, des Lettres et des Beaux-Arts de Belgique, Bulletins, série III. tomes i.–v. 8vo. *Bruxelles*, 1881–83 — The Academy.

Digitized by Google

The Academy. Bruxelles, Académie Royale des Sciences, Tables générales du recueil des Bulletins, série II. tomes xxi.–l., 8vo. *Bruxelles*, 1883

—— —— Mémoires, tomes xliii. 2, xliv., 4to. *Bruxelles*, 1882

—— —— Mémoires couronnés et mémoires des savants étrangers, tomes xliv., xlv., 4to. *Bruxelles*, 1882–83

—— —— Mémoires couronnés et autres mémoires (collection in 8vo.), tomes xxxi.–xxxv., 8vo. *Bruxelles*, 1881–83

—— —— Annuaire, 1882–83, 16mo. *Bruxelles*, 1883

The Observatory. —— Observatoire Royal, Annuaire, 1884, 16mo. *Bruxelles*, 1883

The Editor. Bulletin des Sciences Mathématiques et Astronomiques, série II. tomes vii. 4–viii. 5, 8vo. *Paris*, 1883–84

The Bureau. Bureau International des Poids et Mesures, Travaux et Mémoires, tome ii., 4to. *Paris*, 1883

Rev. W. J. B. Richards. Byrne, Oliver, Dual Arithmetic, 8vo. *London*, 1864

—— —— part ii., 8vo. *London*, 1867

—— —— Tables of dual logarithms, dual numbers, and corresponding natural numbers, 4to. *London*, 1867

The Society. Calcutta, Asiatic Society of Bengal, Journal, part i. vol. lii. 2–4, and part ii. vol. li. 1–4, 8vo. *Calcutta*, 1883

—— —— Proceedings, 1883, ii.–x., 8vo. *Calcutta*, 1883–84

—— Cambridge Philosophical Society, Transactions, vol. xiii. 3, 4to. *Cambridge*, 1883

—— —— Proceedings, vol. iv. 6, 8vo. *Cambridge*, 1883

The Author. Capron, J. R., Meteorological Observations made at Guildown, Guildford, during 1883, 8vo. *Guildford*, 1884

—— Celoria, E. J., Sopra una deviazione sensibile del filo a piombo esistente fra Milano e Genova, 8vo. *Milano*, 1884

—— Chaney, H. J., Calculations of densities and expansions, 8vo. *London*, 1883

Digitized by Google

Cheyne, C. H. H., An elementary treatise on the planetary theory, with a collection of problems, third edition, edited by the Rev. A. Freeman, 8vo. *London*, 1883 — The Editor.

Christiania, Norske Gradmaalingskommission, Vandstandsobservationer, heft ii., 4to. *Christiania*, 1883 — The Norwegian Government.

——— Norwegisches meteorologisches Institut, Jahrbuch für 1877–81, herausg. von Dr. H. Mohn, 4to. *Christiania*, 1879–82 — The Institute.

Cincinnati Observatory, Publications, No. 7, Observations of Comets, 8vo. *Cincinnati*, 1883 — The Observatory.

Clark, Latimer, Manual of the Transit Instrument, 8vo. *London*, 1884 — The Author.

——— Transit Tables for 1884, 8vo. *London*, 1884 — ———

Coimbra, Ephemerides astronomicas calculadas para o meridian do Observatorio da Universidade, para o anno de 1885, 8vo. *Coimbra*, 1883 — The Observatory.

Comité International des Poids et Mesures, Procès-verbaux des séances de 1883, 8vo. *Paris*, 1884 — The Committee.

Copenhagen, Kongelige Danske Videnskabernes Selskabs Skrifter, 6^te^ Række, naturvidenskabelig og mathematisk Afdeeling, band ii. 4, 5, 4to. *Kjöbenhavn*, 1883 — The Society.

——— Öfversigt, 1860, 1883, 2–1884, 1, 8vo. *Kjöbenhavn*, 1860, 1883–84 — ———

De la Rue, W., and H. W. Müller, Experimental researches on the electric discharge with the chloride of silver battery, part iv., 4to. *London*, 1883 — W. De la Rue, Esq.

Dembowski, Baron H. von, Misure micrometriche di stelle doppie e multiple fatte negli anni 1852–78, vol. i., 4to. *Roma*, 1883 — Mdlle. Dembowski.

De Morgan, A Card of logarithms and antilogarithms. — Mr. Prosser.

Doolittle, C. L., Determination of the latitude and longitude of Wilkes-Barre and of Pottsville, Pennsylvania, 8vo. *Harrisburg*, 1884 — The Author.

Draper, H., Researches on astronomical spectrum photography. Extracts from the original note-books, with an intro- — Prof. E. C. Pickering.

Digitized by Google

duction and description of the apparatus by C. A. Young, and measurements and discussion of the plates by E. C. Pickering, 8vo. *Cambridge, Mass.*, 1884

The Society. Dublin, Royal Irish Academy, Proceedings (Science), series II. vol. iii. 9, 8vo. *Dublin*, 1882

——— ——————————— (Polite Literature and Antiquities), series II. vol. ii. 4, 8vo. *Dublin*, 1882

The Academy. ——————————— Transactions (Science), vol. xxviii. 11–13, 4to. *Dublin*, 1882–83

——— ——————————— (Polite Literature and Antiquities), vol. xxvii. 5, 4to. *Dublin*, 1882

The Society. ——— Royal Society, Scientific Transactions, new series, vol. ii. 10–12; vol. iii. 3, 4to. *Dublin*, 1880–84

——— ——————————— Various Nos. of Journal and Scientific Proceedings to complete set. 8vo.

The Earl of Crawford. Dun Echt Observatory Circulars, Nos. 78–83, 4to. *Dun Echt*, 1883–84

The Author. Eastman, J. R., Internal contacts in transits of the inferior planets, 8vo. *Salem, Mass.*, 1883

The Editor. Eder, J. N., The chemical effect of the spectrum. Translated and edited by Capt. W. de W. Abney, 8vo. *London*, 1883

——— English Mechanic, The, and World of Science, Nos. 950–1003, fol. *London*, 1883–84

P. L. H. Davis, Esq. English Pilot, The, fol. *London*, 1755

Liverpool Astronomical Society. Espin, Rev. T. E., A catalogue of the magnitudes of 500 stars determined from photographs taken with the equatorial stellar camera. 8vo. *Liverpool*, 1884

The International Geodesic Association. Europe, Permanent-Commission der Europäischen Gradmessung, Verdhandlungen der im Haag vereinigten p. C., herausgegeben vom Centralbureau der Europäischen Gradmessung, 4to. *Berlin*, 1883

The Author. Ferrari, G. S., Nuove ricerche intorno alla relazione fra i massimi e minimi delle macchie solari e le straordinarie perturbazioni magnetiche, communicazioni I.–XII. 4to. *Roma*, 1874–81

Digitized by Google

Ferrari, G. S., Sulle protuberanze e le macchie solari, osservate nel 1877 nell' osservatorio del Collegio Romano, 4to. *Roma*, 1879 — The Author.

——— Le macchie solari ed il magnetismo terrestre, 4to. *Firenze*, 1880 ———

——— Risposta ad una critica del Sig. Filippo Keller intorno alle osservazioni della declinazione magnetica fatte all' osservatorio del Collegio Romano, negli anni 1875, 1877, 4to. *Roma*, 1880 ———

——— Studiata secondo le osservazioni fatte dal 1875 al 1879 all' osservatorio di Zi-Ka-Wei nella Cina, dal P. Marco Dechevrens, 4to. *Roma*, 1880 ———

——— I recenti progressi dell' Astronomia, 8vo. *Roma*, 1882 ———

——— Il calendario Gregoriano, edizione II., 8vo. *Roma*, 1882 ———

——— La riforma Gregoriana del Calendario, 8vo. *Roma*, 1883 ———

Fiévez, Ch., Etude de la région rouge (A–C) du spectre solaire, 4to. *Bruxelles*, 1883 ———

Forest, E. L. de, On an unsymmetrical law of error in the position of a point in space, 8vo. ———

France, Enquêtes et documents relatifs à l'enseignement supérieur. VIII. Rapport sur les observations de province, 8vo. *Paris*, 1883 — The Academy of Sciences, Paris.

Gautier, E., Nouvelle organisation des observations météorologiques à Genève, au Grand Saint-Bernard et à Martigny, 8vo. *Genève*, 1884 — The Author.

Geneva, Observatoire, Résumé météorologique de l'année 1882 pour Genève et le Grand Saint-Bernard, par A. Kammermann, 8vo. *Genève*, 1883 — The Observatory.

——— Société de physique et d'histoire naturelle, Mémoires, tome xxviii. 1, 4to. *Genève*, 1882 — The Society.

Geological Society, Quarterly Journal, Nos. 155–158, 8vo. *London*, 1883–84 ———

Glasgow, Mitchell Library, Report, 1883, 8vo. *Glasgow*, 1884 — The Library.

Digitized by Google

The Society. Glasgow Philosophical Society, Proceedings, vol. xiv. 8vo. *Glasgow*, 1883

M. E. von Gothard. Gothard, J., Egy új Spektroscop, 8vo. *Buda Pest*, 1884

M. A. von Gothard. ——— S., Adatok *Jupiter* és *Mars* Bolygók physikájához, 8vo. *Buda Pest*, 1884

The Society. Göttingen, Königliche Gesellschaft der Wissenschaften, Abhandlungen, Band xxx., 4to. *Göttingen*, 1883

——— ——————————— Göttingische gelehrte Anzeigen, 1883, 2 vols., 8vo. *Göttingen*, 1883

——— ——————————— Nachrichten von der k. Gesellschaft der Wissenschaften und der Georg-August's Universität, 1883, 8vo. *Göttingen*, 1883

The Glasgow Observatory. Grant, R., Catalogue of 6,415 stars for the epoch 1870, deduced from observations made at the Glasgow University Observatory during the years 1860 to 1881, 4to. *Glasgow*, 1883

The Observatory. Greenwich, Royal Observatory, Astronomical, magnetical, and meteorological observations made in the year 1881, under the direction of Sir G. B. Airy and W. H. M. Christie, 4to. *London*, 1883

——— ——————————— Astronomical results, 1881, 4to. *London*, 1883

——— ——————————— Magnetical and Meteorological results, 1881, 4to. *London*, 1883

——— ——————————— Rates of chronometers on trial for purchase by the Board of Admiralty, 1883, 4to. *London*, 1883

——— ——————————— Report of the Astronomer Royal to the Board of Visitors, 1884, June 7, 4to. *London*, 1884

A. C. Ranyard, Esq. Gregory, D., Catoptricæ et Dioptricæ sphæricæ elementa, 8vo. *Edinburgi*, 1713

The Author. Guimaraens, A. de Souza, On fluid cavities in meteorites, 8vo. *London*, 1883

Musée Teyler. Haarlem, Musée Teyler, Archives, série II. 4me partie, 8vo. *Haarlem*, 1883

Digitized by Google

Harley, Rev. T., Lives which live, a series of biographical reviews. I. Sir John Herschel, 8vo. *London*, 1884 — A. C. Ranyard, Esq.

Hartwig, E., Der grosse Septembercomet von 1882, und sein Begleiter vom 9. October, 4to. *Kiel*, 1883 — The Author.

Harvard College Astronomical Observatory, 38th annual report of the Director, E. C. Pickering, 8vo. *Cambridge, Mass.*, 1884 — The Observatory.

Hasselberg, B., Untersuchungen über das zweite Spectrum des Wasserstoffs, Abhandlung ii., 4to. *St. Petersburg*, 1883 — Academy of Sciences, St. Petersburg.

Helsingfors, Société des Sciences de Finlande, Acta Societatis Scientiarum Fennicæ, tome xii., 4to. *Helsingforsiæ*, 1883 — The Society.

——— Öfversigt af Finska Vetenskaps-Societens Förhandlingar, xxiv. 1881, 8vo. *Helsingfors*, 1882 ———

——— Observations météorologiques, vol. viii., année 1880, 8vo. *Helsingfors*, 1883 ———

——— Bidrag till Kännedom af Finlands Natur och Folk, Heft xxxvii., 8vo. *Helsingfors*, 1882 ———

——— Le Grand Duché de Finlande, notice statistique par K. E. F. Ignatius, 8vo. *Helsingfors*, 1878 ———

Herény, Astrophysikalisches Observatorium, Publikationen, Heft i., herausgegeben von E. von Gothard, 4to. *Herény*, 1884 — M. E. von Gothard.

Herschel, Sir J., Outlines of astronomy, translated into Chinese by A. Wylie, 2nd edition, fol. *Shang Hae*, 1874 — T. Jenner, Esq.

Hirsch, A., et E. Plantamour, Nivellement de précision de la Suisse, exécuté par la Commission géodésique fédérale, livraison 8, 4to. *Genève*, 1883 — The Authors.

Hong Kong Observatory, Report from the Government Astronomer, fol. *Hong Kong*, 1883 — The Observatory.

Horological Journal, The, a monthly paper for the advancement of chronometer, watch, and clock making, Nos. 299–310, 8vo. *London*, 1883–84 — The British Horological Institute.

Digitized by Google

The University, Berne. — Huber, G., Anwendungen der conformen Abbildung, 8vo. *Zürich*, 1883

The Survey. — India, Great Trigonometrical Survey, Account of the operations of. Vol. ix. Electro-telegraphic longitude operations during 1875–77 and 1880–81, by Lieut.-Col. W. M. Campbell and Major W. J. Heaviside, 4to. *Dehra Dun*, 1883

——— — ——— General Report on the operations during 1881–82, prepared under the superintendence of Lieut.-General J. T. Walker, fol. *Calcutta*, 1883

The Meteorological Office. — International Meteorological Committee, Report of the second meeting, held at Copenhagen, 1882, 4to. *London*, 1883

——— — International Polar Commission, Mittheilungen, Heft v., 4to. *St. Petersburg*, 1884

Maxwell Hall, Esq. — Jamaica weather report for April 1883, fol. *Kingston*, 1883

The Author. — Janssen, J., Observations de l'éclipse totale du 6 mai 1883 à l'île Caroline, 8vo. *Salem, Mass.*, 1883

T. Jenner, Esq. — Jenner, T., Mnemonic Geography, part i. (The provinces of China), 8vo. *London*, 1869

The Editor. — Journal of Science, third series, Nos. 115–126, 8vo. *London*, 1883–84

The Committee. — Kew Observatory, Report of the Kew Committee for the year ending Oct. 31, 1883, 8vo. *London*, 1883

The Author. — Kleiber, J. A., Astronomical theory of shooting stars [in Russian], 8vo. *St. Petersburg*, 1884

——— — Klinkerfues, W., Theoretische Astronomie, 8vo. *Braunschweig*, 1871

——— — Knobel, E. B., The chronology of star catalogues, 4to. *London*, 1877

——— — ——— Notes on a Persian MS. of Ulugh Beigh's catalogue of stars, 8vo.

H. Sadler, Esq. — Knowledge, an illustrated magazine of science, Nos. 79–136, 4to. *London*, 1883–84

The Observatory, Göttingen. — Kobold, H., Klinkerfues'sche Constanten zur Reduction auf den scheinbaren Ort für die mittleren Tage 1884, 12^h m. Z., Berlin, 8vo. *Göttingen*, 1883

Digitized by Google

Krueger, A., Zonenbeobachtungen der Sternen zwischen 55° und 65° nördlicher Declination, angestellt auf den Sternwarten zu Helsingfors und Gotha, 4to. *Helsingfors*, 1883 — The Observatory, Helsingfors.

Lamp, J., Neue Berechnung der Parallax von 61 *Cygni* aus den Beobachtungen von Schweizer in Moskau, 1863–66, 8vo. *Kiel*, 1883 — The Author.

Langley, S. P., Experimental determination of wave-lengths in the invisible prismatic spectrum, 8vo. *New Haven*, 1884 — ———

Leeds Philosophical and Literary Society, annual report for 1882–83, 8vo. *Leeds*, 1883 — The Society.

Lindemann, E., Zweites Verzeichniss neuer rother Sterne, 8vo. *St. Petersburg*, 1883 — The Observatory, Pulkowa.

Lindstedt, A., Beitrag zur Integration der Differentialgleichungen in der Störungstheorie, 4to. *St. Petersburg*, 1883 — Academy of Sciences, St. Petersburg.

Liverpool Astronomical Society, Abstracts of the Proceedings, vol. i., ii. session 1882–84, 8vo. *Liverpool*, 1883–84 — The Society.

——— Transactions, Nos. 1, 2, 8vo. *Liverpool*, 1883–84 — The Society.

——— Free Public Library, 31st annual report, 8vo. *Liverpool*, 1884 — The Library.

——— Literary and Philosophical Society, Proceedings, vols. xxxv.–xxxvii. (1880–83), 8vo. *Liverpool*, 1881–83 — The Society.

Leipzig, Astronomische Gesellschaft, Publicationen, xvii. (Mittlere Oerter von 83 südlichen Sternen für 1875·0, von A. Auwers), 4to. *Leipzig*, 1883 — ———

——— Vierteljahrsschrift, Jahrgang xviii. 1–xix. 1, 8vo. *Leipzig*, 1883–84 — ———

——— Königlich-sächsische Gesellschaft der Wissenschaften, Abhandlungen der mathematisch-physischen Classe, Band xii. 9, 8vo. *Leipzig*, 1883 — ———

——— Berichte über die Verhandlungen; mathematisch-physische Classe, 1882, 8vo. *Leipzig*, 1883 — ———

Loewy, M., Description sommaire d'un nouveau système d'Equatoriaux et de son installation à l'Observatoire de Paris, 4to. *Paris*, 1883 — The Author.

Digitized by Google

The Author. Loewy, M., Nouvelles méthodes pour la détermination de la position relative de l'Equateur instrumental par rapport à l'Equateur réel, et des déclinaisons absolues des étoiles et de la latitude absolue, 4to. *Paris*, 1883

——— ——— Deux méthodes nouvelles pour la détermination des ascensions droites des étoiles polaires et de l'inclinaison de l'axe d'un instrument méridien au-dessus de l'Equateur, 4to. *Paris*, 1883

——— ——— Méthode nouvelle pour la détermination des ascensions droites et des déclinaisons absolues des étoiles, 4to. *Paris*, 1883

The Observatory, Pulkowa. Logarithmen (Vierstellige) der trigonometrischen Functionen in Zeit ausgedrückten Winkel, 4to.

The Author. Loomis, Contributions to meteorology, 19th paper, 8vo. *New Haven*, 1883

——— Lorenzoni, G., Sulle determinazioni di Tempo eseguite ad Arcetri nell' autunno del 1882, 8vo. *Venezia*, 1884

——— Lynn, W. T., Celestial motions, a handy book of astronomy, 1st and 2nd editions, 8vo. *London*, 1884

——— Mackinlay, Capt. G., The transit of *Venus* in 1882, 8vo. *Woolwich*, 1883

The Society. Manchester Literary and Philosophical Society, Memoirs, series III. vol. vii. 8vo. *Manchester*, 1882

——— ——— Proceedings, vols. xx.–xxii., 8vo. *Manchester*, 1881–83

——— ——— A centenary of Science in Manchester, in a series of notes, by R. Angus Smith, 8vo. *Manchester*, 1883

C. L. Prince, Esq. Manilius. The Sphere of Marcus Manilius made an English poem, with annotations and an astronomical appendix, by Edward Sherburne, fol. *London*, 1675

The Academy, Buda Pest. Mathematische und naturwissenschaftliche Berichte aus Ungarn, redigirt von J. Fröhlich, Band i., 8vo. *Berlin*

The Observatory. Melbourne Observatory, Monthly record of results of observations in meteorology, terrestrial magnetism, &c. &c.,

Digitized by Google

taken during May 1882–Dec. 1883, under the superintendence of R. L. J. Ellery, 8vo. *Melbourne*, 1882–84

Melbourne Observatory, 18th report of the Board of Visitors, together with the Annual Report of the Government Astronomer, fol. *Melbourne*, 1883 — The Observatory.

——— Royal Society of Victoria, Transactions and Proceedings, vol. xix., 8vo. *Melbourne*, 1883 — The Society.

Meteoriten-Kreisreihen als Erzeuger der Kometen, Sonnenflecken, des Erdmagnetismus, &c., 8vo. (Anon.) — The Author.

Meteorological Office, Hourly readings, 1881, 4–1882, 1, 4to. *London*, 1883 — The Meteorological Office.

——— Quarterly weather report, 1877, appendices and plates, 4to. *London*, 1883 — ———

——— Monthly weather report, 1884, 4to. *London*, 1884 — ———

——— Meteorological atlas of the British Isles, 4to. *London*, 1883 — ———

——— Sunshine records of the United Kingdom for 1881, 8vo. *London*, 1883 — ———

——— Report on the meteorology of Kerguelen Island (note on), 4to. *London* — ———

——— Report of the Meteorological Council of the Royal Society for the year ending March 31, 1883, 8vo. *London*, 1884 — ———

Milan, Reale Osservatorio di Brera, pubblicazioni, Nos. xvii. xxii. xxiii., fol. *Milano*, 1882–84 — The Observatory.

Millosevich, E., Riflessioni sulle carte celesti particolareggiate, e sulle zone speciali di cielo, 4to. *Roma*, 1883 — The Author

Moore, Lieut. E. K., Method of testing chronometers at the U.S. Naval Observatory, 8vo. *Annapolis*, 1884 — The U.S. Naval Institute.

Moncalieri, Osservatorio centrale del Real Collegio Carlo Alberto, Bolletino mensuale, ser. II. vols. iii. 1–iv. 1, 4to. *Torino*, 1883–84 — Italian Meteorological Association.

Montigny, Ch., De l'influence de l'état de l'atmosphère sur l'apparition des couleurs dans la scintillation des étoiles, au point de vue de la prévision du temps, 8vo. *Bruxelles*, 1884 — The Author.

Digitized by Google

The Author. Montigny, Ch., Notice sur la scintillation des étoiles dans ses rapports avec la constitution de leur lumière, d'après l'analyse spectrale, 8vo. *Bruxelles*, 1883

——— ——————— Influence des perturbations magnétiques sur la scintillation des étoiles, 8vo. *Bruxelles*, 1883

The Academy. Montpellier, Académie des Sciences et Lettres, Mémoires de la Section des Sciences, tome x. fasc. ii., 4to. *Montpellier*, 1882

The Author. Morrison, J., An elementary treatise on plane trigonometry, 8vo. *Toronto*, 1880

The Academy. Munich, Königlich-bayerische Akademie der Wissenschaften, mathematisch-physikalische Classe, Abhandlungen, Band xiv. 2, 3, 4to. *München*, 1883

——— ——————————————— Sitzungsberichte, 1883, i., ii., 8vo. *München*, 1883

——— ——————————————— Gedächtnissrede auf Otto Hesse, von Gustav Bauer, 4to. *München*, 1882

The Observatory. Natal Observatory, Report of the Government Astronomer, fol. *Durban*, 1883

The Editor. Naturforscher, Der, Jahrgang xvi. 23–xvii. 24, 4to. *Berlin*, 1883–84

The Lords Commissioners of the Admiralty. Nautical Almanac and Astronomical Ephemeris for the year 1887, 8vo. *London*, 1883

The Society. Neuchatel, Société des Sciences Naturelles, Bulletin, tome xiii., 8vo. *Neuchatel*, 1883

The Author. Newcomb, Simon, Report to the Secretary of the Navy on recent improvements in astronomical instruments, 8vo. *Washington*, 1884

——— Nobile, A., Terza determinazione della latitudine geografica del R. Osservatorio di Capodimonte, con esame delle osservazioni fatte il 1820 da Carlo Brioschi, 4to. *Napoli*, 1883

——— Normand, J. A., Navigation stellaire, 4to. *Paris*, 1883

The Norwegian Government. Norske Nordhavs-Expedition, 1876–78, x., Meteorologi, af H. Mohn, 4to. *Christiania*, 1883

The Author. Nursingrow, A. V., Results of meteorological observations at Daba Gardens, Vizagapatam, 1882, 8vo. *Calcutta*, 1883

Digitized by Google

Nyrén, M., L'aberration des étoiles fixes (Mém. Acad. Imp. de St.-Pétersbourg, ser. VII. tome xxxi. 9), 4to. *St.-Pétersbourg*, 1883 — The Society.

Observatory, The, a monthly Review of Astronomy, Edited by E. W. Maunder, Nos. 75–86, 8vo. *London*, 1883–84 — The Editor.

O-Gyalla, Astrophysikalisches Observatorium, Beobachtungen, Band v., Beobachtungen vom Jahre 1882, herausgegeben von N. von Konkoly, 4to. *Halle*, 1883 — Dr. von Konkoly.

Olsen, O. T., The Fisherman's Nautical Almanac, 1884, 8vo. *Grimsby*, 1884 — The Editor.

Ottawa, Report of the Meteorological Service of the Dominion of Canada for 1881, 8vo. *Ottawa*, 1883 — The Canadian Meteorological Office.

Oxford, Radcliffe Library, Catalogue of books added during the year 1883, 4to. *Oxford*, 1884 — The Radcliffe Trustees.

——— Radcliffe Observatory, Results of Astronomical Observations made in the year 1880, under the superintendence of E. J. Stone, vol. xxxviii., 8vo. *Oxford*, 1883 — ———

——— Report of the Radcliffe Observer to the Board of Trustees, 1883, 8vo. *Oxford*, 1883 — ———

Palermo, Real Osservatorio, pubblicazioni, 1882–83, 4to. *Palermo*, 1883 — The Observatory.

Paris, Académie des Sciences, Comptes-Rendus hebdomadaires des séances, tomes xcvi. 23–xcviii. 22, 4to. *Paris*, 1883–84 — The Academy.

—— Bureau des Longitudes, Annales, travaux faits à l'Observatoire astronomique de Montsouris, tome ii., 4to. *Paris*, 1882 — Bureau des Longitudes.

——————— Annuaire pour l'an 1884, 16mo. *Paris*, 1884 — M. J. Janssen.

——————— Connaissance des Temps ou des mouvements célestes pour l'an 1885, 8vo. *Paris*, 1883 — Bureau des Longitudes.

——————— Ephémérides des étoiles de culmination lunaire et de longitude pour 1884, par M. Loewy, 4to. *Paris*, 1883 — ———

Digitized by Google

Ecole Polytechnique. Paris, Ecole Polytechnique, Journal, cahiers lii.–liii., 4to. *Paris*, 1882–83

The Observatory. —— Observatoire, Annales, publiées sous la direction de M. le Contre-Amiral Mouchez, Mémoires, tome xvii., 4to. *Paris*, 1883

—— ———————————— Observations, 1879–80, 4to. *Paris*, 1882–83

—— ———————————— Atlas écliptique, par MM. Paul et Prosper Henry, Cartes, Nos. 8, 10, 10*a*, 33, 38, 39, 43, 48, 59, 67, 68, 69 *Paris*

—— ———————————— Rapport Annuel pour l'année 1882–83, par M. le Contre-Amiral Mouchez, 4to. *Paris*, 1883–84

The Society. —— Société Mathématique de France, Bulletin, tomes xi. 2–xii. 1, 8vo. *Paris*, 1883–84

—— —— Société Philomathique, Bulletin, série VII. tomes vii. 2–viii. 2, 8vo. *Paris*, 1883–84

The U.S. Naval Observatory. Paul, H. M., A determination of the semidiameter of the Moon from two occultations of the *Pleiades*, July 6, 1877, and September 6, 1879, 4to. *Washington*, 1883

The Editor. Pechüle, C. F., Expédition Danoise pour l'observation du passage de *Vénus*, 1882, 8vo. *Copenhague*, 1883

The Author. Peters, C. F. W., Die gegenseitige Lage der Sternwarten zu Altona und Kiel, 4to. *Kiel*, 1884

The Society. Philadelphia, American Philosophical Society, Transactions, new series, vol. xvi. 1, 4to. *Philadelphia*, 1883

—— ———————————— Proceedings, Nos. 112–114, 8vo. *Philadelphia*, 1882–84

The Institute. ———— Franklin Institute, Journal, series III. vols. lxxxv. 6–lxxxvii. 6, 8vo. *Philadelphia*, 1883–84

The Society. Photographic Society of Great Britain, Journal and Transactions, new series, vols. vii. 9–viii. 8, 8vo. *London*, 1883–84

—— Physical Society of London, Proceedings, vols. v. 4–vi. 1, 8vo. *London*, 1883–84

The Author. Pickering, E. C., Sir William Herschel's observations of variable stars, 8vo.

Digitized by Google

Potsdam, Astrophysikalisches Observatorium, Publicationen, Band iii., 4to. *Potsdam*, 1883 — The Observatory.

Prag, K. K. Sternwarte, Astronomische, magnetische und meteorologische Beobachtungen im Jahre 1882, 4to. *Prag*, 1883 — ———

Pratt, H., Abnormal appearance of *Jupiter's* satellite IV. while in transit on March 12, 1884, 8vo. — The Author.

——— The belts of *Saturn*, 8vo. — ———

Prince, C. L., A summary of a meteorological journal, 1883. — ———

Puschl, C., Ueber die latente Wärme der Dämpfe, 3. Aufl., 8vo. *Wien*, 1883 — ———

Rebeur-Paschwitz, E. von, Ueber die Bewegung der Kometen im widerstehenden Mittel, mit besonderer Rücksicht der sonnennahen Kometen, 4to. *Berlin*, 1883 — ———

Revue Scientifique, série III. tomes xxxii. 18–xxxiii. 5, 4to. *Paris*, 1883–84 — The Society.

Riccò, A., Osservazioni astrofisiche del pianeta Giove, eseguite nel Regio Osservatorio di Palermo, 4to. *Roma*, 1882 — The Author.

——— della grande cometa del 1882 eseguite nel Regio Osservatorio di Palermo, 4to. *Roma*, 1883 — ———

Rigaud, J., A memoir of Stephen Peter Rigaud, 8vo. *Oxford*, 1883 — ———

Rio de Janeiro, Observatoire Impérial, Bulletin astronomique et météorologique, 1883, Nos. 5–11, 4to. *Rio de Janeiro*, 1883 — The Observatory.

Rogers, W. A., On a method of determining the index error of a meridian circle. Studies in metrology, part i. On the reduction of the different star catalogues to a common system, 8vo. *Boston*, 1883 — The Author.

Rome, Accademia Pontificia de' nuovi Lincei, Atti, anno xxxiv. sessione vi.–anno xxxv. sessione v., 4to. *Roma*, 1881–82 — The Academy.

——— Reale Accademia dei Lincei, Atti, anno 279, 1881–82, serie III., Memorie della classe di scienze fisiche, matematiche e naturali, vols. xi.–xiii, 4to. *Roma*, 1882 — ———

Digitized by Google

The Academy. Rome, Reale Accademia dei Lincei, Atti, serie III. anno 230–231, Transunti, vols. vii. 11–viii. 10, 4to. *Roma*, 1883–84

Padre Ferrari. ——— Pontificia Università Gregoriana, vols. xx. 2–xxi. 2–9, fol. *Roma*, 1881–84

The Society. ——— Società degli Spettroscopisti Italiani, Memorie, vols. xii. 4–xiii. 3, 4to. *Roma*, 1883–84

——— ——— Società Italiana delle Scienze, Memorie, serie III. tome iv. v., 4to. *Napoli*, 1882

Italian Meteorological Office. ——— Ufficio Centrale di Meteorologia Italiana, serie II. vol. iii. 1, 4to. *Roma*, 1882

The Society. Royal Asiatic Society of Great Britain and Ireland, Journal, new series, vols. xv. 3–xvi. 2, 8vo. *London*, 1883–84

——— ——— Bombay branch, Journal, Nos. 41, 42, 8vo. *Bombay*, 1883–84

——— ——— Geographical Society, Proceedings and Monthly Record of Geography, new monthly series, vols. v. 7–vi. 6, 8vo. *London*, 1883–84

The Institution. ——— Institution of Great Britain, Proceedings, vol. x. 2, 8vo. *London*, 1883

——— ——— List of members, 1883, 8vo. *London*, 1883

The Society. ——— Meteorological Society, Quarterly Journal, new series, Nos. 46–49, 8vo. *London*, 1883–84

——— ——— Meteorological Record for the quarters ending March 31–Sept. 30, 1883, by William Marriott, Nos. ix.–xi., 8vo. *London*, 1883–84

——— ——— Society of London, Proceedings, Nos. 225–230, 8vo. *London*, 1883–84

——— ——— Catalogue of the scientific books in the Library, General Catalogue, 8vo. *London*, 1883

The Institution. ——— United Service Institution, Journal, Nos. cxx.–cxxiii., 8vo. *London*, 1883–84

The Society. Rugby School Natural History Society, Report for the years 1882–83, 8vo. *Rugby*, 1883–84

The Author. Russell, H. C., The spectrum and appearance of the recent Comet [*b*, 1881], 8vo. *Sydney*, 1882

Digitized by Google

Russell, H. C., Anniversary address to the Royal Society of New South Wales, 1882, 8vo. *Sydney*, 1883 — The Author.

——— The Sydney Observatory, history and progress, 8vo. *Sydney*, 1882 — ———

——— Results of rain and river observations made in New South Wales during 1882, 8vo. *Sydney*, 1883 — ———

St.-Pétersbourg, Académie Impériale des Sciences, Bulletin, tomes xxviii. 4, xxix. 1, 2, 4to. *St.-Pétersbourg*, 1883–84 — The Academy.

——— Mémoires, tomes iii. 5, vi. 11, 4to. *St.-Pétersbourg*, 1860–63 — ———

San Fernando, Almanaque Náutico para 1884–85, calculado en el Instituto y Observatorio de Marina de la Ciudad de San Fernando, 8vo. *Barcelona*, 1882–83 — The Observatory.

——— Instituto y Observatorio de Marina, Anales, seccion 2, observaciones meteorologicas, año 1882, fol. *San Fernando*, 1883 — ———

Schwarz, Bernhard, Astronomische Untersuchung über eine von Archilochus und eine in einer assyrischen Inschrift erwähnte Sonnenfinsterniss, 8vo. *Wien*, 1883 — The Author.

Science Monthly, The, vol. i. No. 2, 4to. *London*, 1884 — The Editor.

Science, Nos. 17–68, 4to. *Cambridge, U.S.*, 1883–84 — ———

Scientific Roll, The, and magazine of systematised notes, conducted by Alex. Ramsay, No. 11, 8vo. *London*, 1883 — ———

Searle, A., The Zodiacal Light, 8vo. *Boston*, 1883 — The Author.

Secchi, P. Angelo, Progetto di un monumento alla memoria del, 4to. *Roma*, 1883 — Padre Ferrari.

Sharpless, I., On the latitude of Haverford College Observatory, 8vo. *Philadelphia*, 1883 — The Author.

Sidereal Messenger, The, edited by W. W. Payne, vol. ii. 4–No. 24, 8vo. *Northfield, Minn.*, 1883–84 — The Editor.

Siemens, Sir C. W., On the conservation of solar energy (reply to Mr. E. H. Cooke), 8vo. *London*, 1883 — The Author.

——— On the dependence of radiation on temperature, 8vo. *London*, 1883 — ———

Sirius, Zeitschrift für populäre Astronomie, Bände xv. 6–xvii. 5, 8vo. *Leipzig*, 1883–84 — The Editor.

Digitized by Google

The Society. Society of Arts, Journal, Nos. 1595–1647, 8vo. *London*, 1883–84

——— ——— for Psychical Research, Proceedings, vol. i. Nos. 2, 3, 8vo. *London*, 1883

Prof. Bredichin. Socoloff, A., Sur la queue du I^er^ type de la comète de 1858, V., 8vo. *Moscou*, 1884

The Society. South African Philosophical Society, Transactions, vol. ii. part iii., 8vo. *Cambridge*, 1882

The Observatory, Pulkowa. Struve, H., Zur Theorie der Talbot'schen Linien, 4to. *St.-Pétersbourg*, 1883

——— ——— L., Resultate aus den in Pulkowa angestellten Vergleichungen von *Procyon* mit benachbarten Sternen, 4to. *St. Petersburg*, 1883

——— ——— Otto, Rapport sur le mémoire de M. Backlund, Untersuchungen über die Bewegung des Encke'schen Cometen, 1871–81, 8vo. *St. Petersburg*, 1883

——— ——— Ueber einen neuentdeckten Cometen, 8vo. *St. Petersburg*, 1883

——— ——— Sur le mémoire de M. Nyrén, L'aberration des étoiles fixes, 8vo. *St.-Pétersbourg*, 1883

The Society. Sydney, Royal Society of New South Wales, Journal and proceedings, 1882, vol. xvi., 8vo. *Sydney*, 1883

The Observatory. Tacubaya (Chapultepec), Anuario del Observatorio nacional para el año de 1884, 16mo. *Mexico*, 1883

The Society. Tasmania, Royal Society of, Papers and proceedings, and report for 1881, 8vo. *Tasmania*, 1882

The Author. Terby, F., Remarques *à propos* des récentes observations de M. Schiaparelli sur la planète *Mars*, 8vo. *London*, 1882

——— ——— Note sur l'Aurore Boréale du 2 Oct. 1882, 8vo. *Bruxelles*, 1882

——— ——— Note sur l'Aurore Boréale du 17 Nov. 1882, 8vo. *Bruxelles*, 1882

——— ——— Aspect de la grande comète de 1882, observée à Louvain, 8vo. *Bruxelles*, 1882

——— ——— Deuxième notice. 1882

——— ——— Troisième notice. 1883

Digitized by Google

Terby, F., Sur l'existence et sur la cause d'une périodicité mensuelle des Aurores Boréales, 8vo. *Bruxelles*, 1883 — The Author.

——— Observations de la Lumière Zodiacale, et d'un petit bolide, à Louvain, 8vo. *Bruxelles*, 1883 — ———

Tiflis, physikalisches Observatorium, Magnetische Beobachtungen, 1881–82, 4to. *Tiflis*, 1883 — The Observatory.

——— Meteorologische Beobachtungen, 1882, 4to. *Tiflis*, 1883 — ———

Tischner, A., The sun changes its position in space; therefore it cannot be regarded as being in a condition of rest, 16mo. *Leipzig*, 1883 — The Author.

Tokio, Seismological Society of Japan, Transactions, vol. vi. Jan.–June 1883, 8vo. *Tokio*, 1883 — The Society.

——— University, Memoirs of the science department, No. 5 (appendix), Measurement of the force of gravity at Sapporo (Yesso); No. 9, Earthquake measurement, by J. A. Ewing, 4to. *Tokio*, 1882–83 — The University.

Toronto, Canadian Institute, Proceedings, vols. i. 4–ii. 1, 8vo. *Toronto*, 1883–84 — The Institute.

——— Observatory, Report of the Canadian observations of the transit of *Venus*, 1882, Dec. 6, 8vo. *Toronto*, 1884 — The Observatory.

Toulouse, Académie des Sciences, Inscriptions et Belles-Lettres; Mémoires, tome v., 2^{me} semestre, 8vo. *Toulouse*, 1883 — The Academy.

Turin, Osservatorio della regia Università, Bolletino, anno xxvii. (1882), fol. *Torino*, 1883 — The Observatory.

——— Reale Accademia delle Scienze, Atti, vols. xviii. 5–xix. 3, 8vo. *Torino*, 1883–84 — The Academy.

——— Memorie, serie II. tomo xxxv., 4to. *Torino*, 1884 — ———

——— Il primo secolo della R. Accademia, Notizie storiche e bibliografiche (1783–83), 4to. *Torino*, 1883 — The Academy.

United States, Congressional Directory, 1882, 8vo. *Washington*, 1882 — The U.S. Government.

Digitized by Google

The College. University College, London, Calendar for the session 1883–84, 8vo. *London*, 1883

The Society. Upsala, Regia Societas Scientiarum, Nova Acta, series III. vol. xi. 2, 4to. *Upsaliæ*, 1883

The Academy. Vienna, Kaiserliche Akademie der Wissenschaften, Denkschriften, mathematisch-naturwissenschaftliche Classe, Band xlv. xlvi., 4to. *Wien*, 1883

——— ——— Circulars, Nos. l.–lii., 8vo. *Wien*, 1883–84

The American Ephemeris Office. Washington, American Ephemeris and Nautical Almanac, astronomical papers, vol. ii. 1, 2; vol. iii. 1, 4to. *Washington*, 1883–84

The U.S. Bureau of Navigation. ——— Bureau of Navigation, Telegraphic determination of longitudes in Japan, China, and the East Indies, 4to. *Washington*, 1883

The Observatory. ——— United States Naval Observatory, Astronomical and meteorological observations made during the years 1878–79, 4to. *Washington*, 1882–83

The Society. ——— Philosophical Society, Bulletin, vols. iv.–vi., 8vo. *Washington*, 1883–84

The Institution. ——— Smithsonian Institution, Annual report of the Board of Regents for 1881, 8vo. *Washington*, 1883

——— ——— Miscellaneous Collections, vols. xxii.–xxvii., 8vo. *Washington*, 1882–83

U.S. Chief Signal Office. ——— United States Signal Service, Annual report for the year ending June 30, 1880, parts i. and ii., 8vo. *Washington*, 1881

——— ———Professional papers, Nos. viii.–xii., 4to. *Washington*, 1882–83

The U.S. Coast Survey. ——— Coast and Geodetic Survey, Report of the superintendent, showing the progress of the work during the year ending June 1881, 4to. *Washington*, 1883

The Observatory. Winlock, W. C., Observations of the great comet of 1882, made at the United States Naval Observatory, 4to. *Washington*, 1883

Digitized by Google

Wittram, Th., Allgemeine Jupiterstörungen des Enckeschen Cometen für den Bahntheil zwischen 152° 21′ 7·62″ und 170° wahrer Anomalie, 4to. *St. Petersburg*, 1883 — Academy of Sciences, St. Petersburg.

Wolf, C., Recherches historiques sur les étalons de poids et mesures à l'Observatoire, et les appareils qui ont servi à les construire, 4to. *Paris*, 1882 — The Author.

—— R., Astronomische Mittheilungen, Nos. lix.–lxi., 8vo. *Zürich*, 1883–84 — ——

Yale College Observatory, Report for the year 1882–83, 8vo. *New Haven*, 1883 — The Observatory.

Young, C. A., Astronomical collisions, 8vo. *New York*, 1883 — The Author.

Zoological Society of London, Transactions, vol. xi. 8, 4to. *London*, 1883 — The Society.

—— Proceedings of the scientific meetings, 1882, 4–1883, 4, 8vo. *London*, 1883–84 — ——

—— List of the Fellows, 8vo. *London*, 1883 — ——

—— List of the animals now or lately living in the gardens, 8th edition, 8vo. *London*, 1883 — ——

—— Catalogue of the Library, 8vo. *London*, 1883 — ——

Zürich, Schweizerisches meteorologisches Central-Anstalt, Annalen, 1882 (Schweizerische meteorologische Beobachtungen, Jahrgänge xviii. 5, xix.), 4to. *Zürich*, 1883 — The Institute.

Drawings, Instruments, &c., Presented to the Society.

Astronomical drawings made by the late Mr. Banks, of Ealing. — Mrs. Banks.

A six-prism spectroscope, by Browning. — J. D. Perrins, Esq.

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The Society. Commemorative medal of the Italian Society of Sciences.

Books Purchased with the Turnor and Horrox Funds.

Bulletin Astronomique, 1884, 8vo. *Paris*

Brockmann, F. J., System der Chronologie, 2 vols., 8vo. *Berlin*, 1825

Ideler, L., Handbuch der mathematischen und technischen Chronologie, 2 vols., 8vo. *Berlin*, 1825

International Scientists' Directory, 12mo. *Boston*, 1883

Nature, 1883–84, 4to. *London*

——— Vol. xiv. (to complete set), 4to. *London*, 1875

Newcomb, S., and E. S. Holden, Astronomy (briefer course), 2nd edition, 8vo. *New York*, 1884

Philosophical Magazine, 1883–84, 8vo. *London*

Thomson, Sir W., and P. G. Tait, Treatise on Natural Philosophy, vol. i. part ii., new edition, 8vo. *Cambridge*, 1883

Tschermak, G., Die mikroskopische Beschaffenheit der Meteoriten, Heft i., fol. *Stuttgart*, 1883

Zech, J., Tafeln der Additions- und Subtractions-Logarithmen für sieben Stellen, 2. Aufl., 8vo. *Berlin*, 1863

Zeitschrift für Instrumentenkunde, 1883–84, 4to. *Berlin*

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